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1894.

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Stated Meeting, January 5th, 1893.

A CONTRIBUTION TO CEREBRAL SURGERY.

DIAGNOSIS, LOCALIZATION, AND OPERATION FOR REMOVAL OF THREE TUMORS OF THE BRAIN: WITH SOME COMMENTS UPON THE SURGICAL TREATMENT OF BRAIN TUMORS.

By CHARLES McBURNEY, M.D.,

AND

M. ALLEN STARR, M.D.

THE practical results from the study of cerebral localization in the successful surgical treatment of brain diseases form a subject of growing interest. Every fact which bears upon it and every case in which an operation has been performed should be recorded, whether the result obtained be favorable or the reverse. Successful results are sure to be published. It is very desirable, however, in order that we may learn to properly estimate the dangers and difficulties that may be met with in the diagnosis and treatment of localizable disease, that all results, even those which are in some measure unfavorable, should also be made known. In this paper the surgical treatment of brain tumors is to be considered. The general results of operations hitherto recorded will be reviewed, and our own experience in three cases related.

The number of cases of tumor of the brain in which surgical relief has been attempted, so far as we have been

able to find reports in current literature, and including the three cases presented here, is 87. Seventy-four of these were tumors of the cerebral hemispheres; thirteen of them were tumors of the cerebellum. The results are given in the following table:

	Cerebral.	Cerebellar.	Total.
Total number of cases operated upon, . . .	74	13	87
Cases in which tumor was not found, . . .	16	7	23
" " " " found, but removed,	1	2	3
" " " " removed and patient recovered, . . .	38	2	40
Cases in which tumor was removed and patient died, ...	19	2	21

It will be seen that the percentage of recoveries after the successful localization and removal of the tumor is 46. Considering how recently the facts of localization have been determined, and how novel is the surgical procedure of operation upon the brain, this large percentage of successful results is both interesting and encouraging. Therefore every case in which the presence of a tumor of the brain is suspected should be studied with increased care, and the question of operation should be thoroughly considered.[1]

It seems best to distinguish broadly between tumors of the cerebrum and tumors of the cerebellum. The diagnosis between tumors situated in these different regions is perfectly easy, and the risk of operation is so different as to demand their separate consideration.

As shown in the table already given, 74 tumors of the cerebral hemispheres have been treated surgically. In 16 of these cases the operation was unsuccessful, either

[1] The literature is extensive. Recent important articles are as follows: Weir and Seguin, American Journal of the Medical Sciences, July, August, and September, 1888; Keen, American Journal of the Medical Sciences, November, 1888; Park, "Surgery of the Brain," Transactions of the Association of American Physicians and Surgeons, 1889; Von Bergmann, "Die chirurgische Behandlung von Hirnkrankheiten," 1889; P. C. Knapp, "Intracranial Growths," 1891 (the last contains tables of all cases up to date of publication); Theodore Diller, The Pittsburg Medical Review, October and November, 1892.

because the tumor was not found by the surgeon at the point at which it was supposed to lie, or because the operation was undertaken for the relief of the symptoms caused by intracranial pressure, and not with a view to the removal of the tumor (8 cases).

In some of the cases the localizing symptoms were clearly insufficient to indicate the position of the tumor, and a cautious neurologist would not have advised an attempt to find it.

In others the localizing symptoms were well marked and the diagnosis seemed clear, yet the tumor really lay at such a depth as to be inaccessible, or infiltrated the brain to such an extent as to make removal impossible.

In 57 cases out of the 74 the tumor was successfully located and removed; 38 of the patients recovered; 19 died. Of these 57 tumors, 34 were removed from the motor (central) region of the brain. It is in this region that the location of a tumor can be most easily determined, and here few mistakes in diagnosis have been made. The occurrence of spasm or of paralysis, limited to one limb or extending from one to the other in a definite order, is diagnostic. In one of the cases reported, the motor symptoms were certainly of the greatest service in determining the location of the tumor. So, too, in a case reported by Erb in July, 1892, in the *Deutsche Zeitschrift für Nervenheilkunde.* This case deserves mention on account of its unique history:

The patient was a male, who had suffered for some months from the general symptoms of brain tumor—namely, headache, vertigo, vomiting, and optic neuritis. The development of occasional spasms, followed by paralysis in the left arm and leg, indicated the central convolutions of the right hemisphere as the probable position of the tumor. Czerny operated in November, 1890, and found the tumor to be an infiltrating gliosarcoma. He removed a part of it—complete extirpation being impossible. The patient recovered from the operation, and was very much improved for eight months, when he began again to suffer from the old symptoms. In Novem-

ber, 1891, it was thought best to repeat the operation, and again a large part of the tumor was removed. Striking improvement followed this second operation; but at the date of the last report, July, 1892, symptoms had returned, and a third operation was under discussion.

This case demonstrates the possibility of relieving the most serious symptoms and of prolonging life even when the entire tumor cannot be removed.

The location of the tumor in the remaining 23 cases was as follows: in the frontal region in 6; in the parietal region in 3; in the occipital region in 1. In the remainder the location of the tumor was not exactly stated in the histories. It is evident, therefore, that tumors have been successfully removed from almost all parts of the convexity of the cerebral hemispheres. It is impossible either to satisfactorily diagnosticate or to remove tumors lying on the median or basal surfaces, and no attempt at such removal has been made.

Surgical treatment has been attempted in 13 cases of cerebellar tumor. In only 2 cases has the operation been successful, and in 7 cases the tumor was not reached. An abstract of all these cases is given at the end of this paper, in order that the difficulties met with may be appreciated.

An operation undertaken with a view to the removal of a tumor of the brain should be especially well considered beforehand. To a much greater extent than is at all frequent in other parts of the body are these operations essentially exploratory, and in spite of the very perfect and constantly increasing knowledge of the character and localization of brain tumors, the operator will probably always be in doubt, until the tumor is reached, as to whether he will be able to remove it or not. Assuming that an accurate diagnosis has been made, and that the localization of the disease has been carefully studied by a skilled neurologist, grave doubt may still exist as to the consistence, the size, the vascularity, even as to whether the tumor is situated on the surface or buried to a con-

siderable depth, and still more as to whether the tumor is encapsulated and easily enucleable, or so infiltrates the brain tissue as to present no definite border. Many exploratory operations in other parts of the body are extremely simple and perfectly safe so long as the work done is confined to the strict nature of an exploration. We enter the peritoneal cavity, explore kidneys through incision in the loin, open joints, and take sections from tumors for examination, with almost entire impunity from accident, so long as nothing more than investigation be made. The reason for this safety lies chiefly in the fact that the exploratory road is not difficult, and that such difficulties as exist are appreciated beforehand and are easily overcome. This statement, however, does not apply to operations upon brain tumors. Unexpected difficulties, even serious accidents, are liable to occur at almost any stage in the operation, so that from beginning to end the exploration itself is an operation of importance. This is better appreciated when we consider the causes of death after these operations, whether the tumor has been reached and removed or not. Excluding complications not immediately dependent on the operation, death has resulted, in 54 per cent of the cases operated upon, either from sepsis, hæmorrhage, or shock, or from two or all of these causes combined. Sepsis need not be especially considered, as there exists no greater difficulty in avoiding it than the surgeon meets with in other parts of the body. Most careful antiseptic precautions should be taken. The head should be completely shaved, and great attention be paid to the thorough cleansing of the scalp, the operation being postponed if it is found that a moist eczema or other discharging surface exists at any point. Hæmorrhage is a most important consideration. It begins from the moment the knife touches the scalp, is enormously different in quantity in different cases, and is liable to be unexpectedly increased at a later stage in the operation after the patient has already lost an important quantity of blood. I am convinced that hæmorrhage

deserves the closest consideration on the part of the operator.

The incisions in the scalp should unquestionably be free. After opening the skull it may be necessary to greatly enlarge the operative field, and this stage is a most unfavorable one for making first incisions in the scalp. It is best to lay bare a bone area at least three inches in diameter. The horseshoe-shaped incision, or one representing more than half of a circle, is most frequently used, and is doubtless the best. Such an incision allows a large flap, including all the integuments excepting the periosteum, to be easily turned back. The hæmorrhage from this incision is very active, and the means adopted for its prevention not entirely satisfactory. The use of the rubber band wound tightly about the head below the field of work is exceedingly disappointing, probably on account of the shape of the skull, the rubber band not making constriction evenly about the whole circumference. Finger pressure over the afferent vessels is also insufficient, for only a few vessels can be compressed, while, owing to the free anastomosis in the scalp, the bleeding comes from every direction. I am inclined to think that the most practical and satisfactory method is to make the whole incision at once, immediately compressing the entire wound with dry gauze, and then, lifting the gauze first at one point and then at another, to seize the vessels in succession with pressure forceps. With good assistance and active work the vessels can all be controlled pretty rapidly.

The next question that arises, after the vessels have been ligated and the very numerous pressure forceps gotten out of the way, is: How shall the skull be entered? A very usual method by which the skull has been opened has been by means of the trephine, either large or small, the opening being afterward enlarged with the rongeur forceps. If a trephine less than an inch and a half in diameter be used, the opening made will not be large enough to treat any brain tumor, excepting a very small

surface growth. The opening has usually been enlarged with rongeur forceps—a great sacrifice of bone, which cannot be entirely safely replaced, being thus made. If a still larger trephine is used, one has a clumsy instrument to work with and still has a large bone defect to close. Ordinarily the button of bone had better not be replaced after the removal of a tumor, for the cavity in the brain frequently needs packing with gauze, and, besides, the button may necrose.

Having frequently made use of another method of entering the skull for extensive operations, I have no hesitation in recommending it to facilitate the removal of brain tumors. I refer to the method suggested by Wagner. It consists essentially in cutting through the skull with the chisel in the line of the scalp incision, the scalp flap not having been turned back. This bone flap with scalp flap still attached is then pried up and turned back, thus exposing as large an area of dura mater as the original incision outlined. The method is practically applied as follows: The horseshoe-shaped scalp incision having been made and all vessels caught and tied, the cut is deepened through the pericranium in the same line, so that this tissue may not be injured when working on the bone. At the centre of the convexity of the incision the pericranium is turned back far enough to permit of the application of a half-inch trephine. This small button of bone is removed to allow of the subsequent use of the elevator. Starting from this trephine opening with a very small gouge, a shallow groove is cut following the line of incision in scalp and pericranium. Cutting this groove prevents subsequent splintering of the surface. Then with a sharp osteotome, held nearly parallel with the surface of the skull, it is not difficult with the mallet to cut through the skull without injuring the dura mater, until, introducing an elevator into the trephine opening, the whole plate of bone can be pried up, the uncut portion of its circumference readily breaking. The whole plate of bone has the pericranium and scalp flap attached, and

opposite the line of fracture, even, the pericranium remains untorn. Slowly lifting the bony plate and feeling beneath it to make sure that vessels or adhesions are not rudely torn, the entire mass can be turned back and the dura exposed. If localization has been fairly correct it will not now be necessary to enlarge the opening. Hæmorrhage from the bone may be, as it was in one of the cases reported, excessive; usually it will cease after short pressure; if not, the larger vessels should be plugged either with gauze or bits of sponge or catgut. Hæmorrhage from the surface of the dura is usually slight, and can be most rapidly stopped with a light touch of the Paquelin cautery, or, if one prefers, a fine curved needle threaded with catgut may be passed under the bleeding point and the vessel tied.

To open the dura I prefer to cut directly down upon it with the knife at a point an eighth or a quarter of an inch away from the bony margin. When an opening is made, small blunt scissors may be introduced, and the whole flap of dura, similar in shape to the bone flap, turned back. Picking up the dura mater with a tenaculum is more likely to be accompanied by a wound of a vessel in the pia mater. Of course, if the dura is adherent, the flap must be cautiously made and reflected. Color, consistence, bulging, and pulsation of the surface of the brain are now all rapidly determined. If the tumor is evident at once and clearly defined, with a capsule, its enucleation with the finger or blunt scissors is not difficult. If tumor tissue is plain, but the growth infiltrates the brain substance, it may be very difficult to decide when to stop in attempting removal. If clearly not removable *in toto*, it may still add much to life and comfort to remove a portion, as was shown in Czerny's case, intracranial pressure being relieved. As a rule, if removal is begun, it had better be completed, for to leave a portion of an infiltrating tumor is to leave the patient with the certainty of recurrence at an early date. If tumor tissue is not seen on the surface, its presence beneath the sur-

face may be suspected from the prominence of, and the absence of pulsation in, the surface tissue. Palpation may define that a solid tumor exists below; or a blunt probe may be pushed in the most likely direction to see if resistance is encountered; or an aspirating needle may be introduced to determine the presence of fluid. If a solid tumor is discovered the convolutions over it should be separated or the brain should be incised, as may be necessary, and removal effected, if possible by enucleation. If a cystic tumor is found it should ordinarily be incised and drained. Drainage can best be effected by the introduction through the incision of a narrow strip of thin rubber tissue which has been folded once or twice upon itself. Fluid flows out by the side of this material perfectly well, and the thin and flexible tissue is incapable of doing harm if thoroughly aseptic. Wounded vessels in the pia mater may be quite troublesome, not so much from the quantity of blood lost as from the amount of time lost in attending to them. Their walls are so delicate that the ligature is applied with difficulty. A light touch with the cautery is the quickest way of disposing of the smaller ones, and the larger ones may be compressed by a bit of sponge in the hand of an assistant, and the cautery or a ligature applied, if necessary, later. The wounding of one of the large venous sinuses, or of a vein near the point of entrance into a sinus, is sure to be followed by abundant hæmorrhage. If the wound in a sinus is large, it had best be immediately packed with gauze. If it is small enough to be closed by one or two pairs of pressure forceps, these may be applied, and left in position, supported by the dressings, for two or three days. It is useless and time-consuming to try to ligature or sew up a wounded sinus.

Free hæmorrhage from the cavity left after removal of a tumor should be met by immediate compression with gauze packing, which may be left in place, if necessary, until a change of dressings is made. In operating for tumor in the cerebellum, the method last recommended

for entering the skull is hardly applicable. The area is too small and the danger of wounding the sinuses too great to allow of the lifting of a bony plate with the thick flap of soft coverings. It is better here to sacrifice the bone, making a small, safe opening with trephine or chisel opposite the centre of the fossa, and then enlarging the opening on all sides with forceps. The fact that the present knowledge of cerebral localization does not enable us to decide whether a cerebellar tumor is a surface or a deep one, renders these operations more especially exploratory. Whether it is safe or justifiable to cut away a considerable portion of cerebellum in order to reach a deep-seated tumor in this region, further experience is needed to decide.

If a tumor has been found and removed, the dura mater would probably be only partially stitched back into place by fine catgut stitches, as to completely close a cavity in the brain in which bleeding was still going on would subject the patient to the dangers of compression. Therefore, usually packing would at first be left in place, or other drainage provided for. If a tumor is not found and no contraindication to complete closure exists, the dural flap may be replaced and stitched all the way round to the narrow border left for that purpose. The advantage of the large, single bone flap is now readily appreciated. The osseous plate, with pericranium undisturbed and integument undissected, is turned back into place and fits accurately. The skin may be stitched all the way round or not, according as drainage must be provided for or is not needed. The small trephine opening is there if a drain is required. If packing has been introduced, the bony plate may be left partly raised like the lid of a box, and only pressed into place after the packing has been removed.

The question of hæmorrhage has been referred to in detail, because blood loss in itself has undoubtedly not a little to do with the death rate after operations for brain tumors. Bleeding is met with in most cases at every stage

in the work, and not only may the sum of the loss be considerable, or even very large, but every moment spent in attending to it prolongs the operation, and so directly contributes to inducing the third cause of fatality mentioned—namely, shock. The length of time occupied and the size of the tumor bear a direct relation, as in the surgery of other parts of the body, to the amount of shock developed. Every effort should, therefore, be made to shorten the time occupied in operating, as the size of the tumor will be accurately known only at a late stage in the operation. No especial means either for the prevention or treatment of shock from operations upon brain tumors exist, other than those with which every surgeon is familiar when he meets with this condition under other circumstances.

CASE I. *Sarcoma of the left frontal lobe; mental and motor symptoms; successful localization and removal; subsequent death.*—C. S., aged forty, a farmer by occupation, of good family history, and of good general health until this illness (with the exception of specific disease acquired at the age of twenty-two, but without subsequent manifestations), was suddenly seized with a convulsion in December, 1890, while driving a cart. He remembers a sudden feeling of dizziness and distress, and then a turning of his head forcibly to the right side; he has no recollection of what followed, but learned that he had been found upon the road, had been picked up and carried home, where he remained unconscious for two hours and a half; he is not sure whether he had a general convulsion. On recovering consciousness he found his right side, including face, arm, and leg, slightly weak, and noticed some difficulty in talking; this condition gradually subsided, so that in two weeks he was able to go back to his work and felt in his usual health. This is the only convulsion or sudden attack of any kind which occurred during his entire illness. But it is from this attack that his illness dates.

The various symptoms which subsequently developed were very gradual in their onset, so that it is quite impossible to fix any dates for particular symptoms. During the six months from January, 1891, to July, 1891, he suffered occasionally from headache and nausea, and in July began to notice that his sight was growing dim and that

the headaches were becoming more and more frequent and intense. Between July, 1891, and January, 1892, the pain became localized over the forehead and top of the head on the left side; it was not particularly worse at night, but at times was very severe. During this period he noticed a progressive dulness of thought, general hebetude, an aversion to work which was unnatural to him, and a slowness of mental activity which he described as increasing stupidity; and increasing difficulty in the use

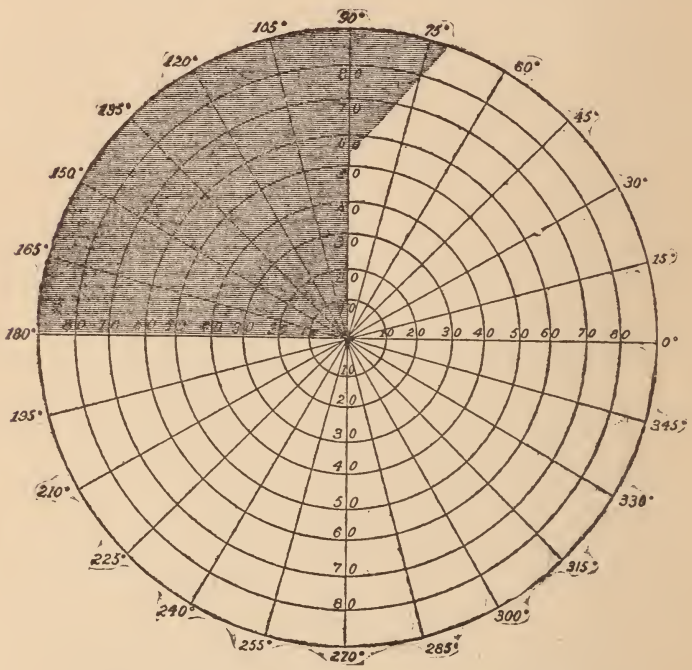

FIG. 1. CASE I.—The visual field of the left eye.

of language, so that it took him longer to express his ideas, there being, however, no difficulty in articulation, and no lack of words.

He also noticed by the close of the year that his right side had become a little weaker than his left side; that his hand was slightly awkward and that his leg felt a little heavy. The symptom, however, which caused him most distress was his gradually increasing dimness of vision, and it was on account of this that he came to New York

from his home in Alabama. He was seen at the New York Eye and Ear Infirmary by Dr. Derby, who discovered a well-marked condition of optic neuritis in both eyes, more marked in the left eye. V.: O. D., = 20/L.; O. S., = 20/XX. In right eye the upper and inner quadrant of visual field wanting. Dr. Derby referred him to the Nervous Department of the Vanderbilt Clinic for confirmation of his diagnosis of cerebral tumor, and also for treatment.

When I first saw him at the Clinic on January 14th, 1892, the following symptoms were present: severe and constant *frontal headache*, located over the top of the head, and more especially over the left side, about at the upper third of the coronal suture, and at this area, over a space about three inches in diameter, there was considerable *tenderness to percussion*. There was no vertigo on rising or on change of position. There was a state of *partial blindness*, due to the very well marked condition of *optic neuritis*, and decidedly worse in the left eye.

There was a condition of *mental dulness* which was noticeable, and which he himself and a friend who accompanied him insisted was wholly unnatural. This dulness consisted in a slowness of thought which made him appear very stupid. It took him some time to appreciate the meaning of questions, and it was an effort to answer them. This effort was not due to any actual disturbance of speech, any loss of words, or any difficulty in pronunciation, though he complained that he could not talk as fluently or as rapidly as heretofore. The condition was, therefore, in no sense an aphasic one, but could only be spoken of as a slowness in mental processes. His comprehension was good and his conclusions were correct, when he had time to think; but rapid mental action was impossible, and if insisted upon he became confused and would say that he could not think. Hence he distrusted his own mental power, and said that he did not think that his judgment was as good as formerly. He was disinclined to occupy himself in any way, and sat in a listless manner, saying nothing, for hours at a time. He would often sleep in the daytime, though at night his sleep was often broken by his pain. It was not easy for him to hold his attention to any subject continuously for any length of time. His mental state might be termed a dull listlessness, and gave the impression that he was a sick man.

Careful examination detected the existence of a *slight*

right hemiplegia; his face was slightly flattened and slow in motion on the right side; his hand was somewhat awkward and clumsy, the power being 140 by the dynamometer, that on the left being 160. He dragged the toe slightly in walking, though his gait was not noticeable. He had great exaggeration of the knee jerks and marked ankle clonus. He complained of a feeling of numbness, both in the hand and foot, but there was no objective anæsthesia.

Diagnosis.—From these symptoms a diagnosis was reached of a brain tumor. The situation of the tumor was not easily determined. The slight right hemiplegia indicated that it was in the left hemisphere of the brain, near to but not within the motor region. The hemiplegia had appeared long after the other symptoms, hence it was evident that the motor zone had been only reached when the tumor had become large. The position of the headache and of the tenderness to percussion over the frontal region, and the existence of the mental symptoms described, appeared to indicate the frontal lobe as the probable situation. This was confirmed by the absence of anæsthesia or hemianopsia or of sensory aphasia, all of which conditions would have been likely to have been present in a tumor situated near to but behind the motor zone in the parietal lobe. The mental symptoms were considered of very great importance in the diagnosis of a frontal lobe lesion.

A study of twenty-three cases of disease of the frontal lobes of the brain,[1] made by one of us in 1884, showed that decided mental disturbances occurred in half of the cases. At that time the following conclusions were reached: "The form of mental disturbance in lesions of the frontal region does not conform to any type of insanity. It is rather to be described as a loss of self-control and a consequent change of character. The mind exercises a constant inhibitory influence upon all action, physical and mental, from the simple restraint upon the lower reflexes, such as the action of the sphincters, to the higher control over the complex reflexes, such as emotional impulses and

[1] Starr: "Cortical Lesions of the Brain." The American Journal of the Medical Sciences, April, 1884.

their manifestation in speech and expression. This action of control implies a recognition of the import of an act in connection with other acts; in a word, it involves judgment and reason, the highest mental qualities. By inhibiting all but one set of impulses it enables one to fix the attention upon a subject and to hold it there. It seems probable that the processes involved in judgment and reason have for their physical basis the frontal lobes; if so, the total destruction of these lobes would reduce man to the state of an idiot; their partial destruction would be manifested by errors of judgment and reason of a striking character. One of the first manifestations would be a lack of that self-control which is the constant accompaniment of mental action, and which would be shown by an inability to fix the attention, to follow a continuous train of thought, or to conduct intellectual processes. It is this very symptom that was present in one-half of the cases collected. It occurred in all forms of lesion—from injury by foreign bodies, from destruction by abscess, from compression and softening due to the presence of tumors—and, therefore, cannot be ascribed to any one form of disease. It did not occur in lesions of other parts of the brain here cited. But its presence in such a large number of these cases warrants the suggestion that in cases of suspected lesion of the frontal lobe the mental condition of the patient, as shown by his acts of judgment and reason, should be carefully examined, and a change of character or behavior accurately noted."

Ferrier, in his Croonian lectures in 1890, again called attention to the occurrence of such mental symptoms in connection with diseases of the frontal lobes.

Welt[1] concluded, from a study of eight cases under his observation, that changes in character and disposition are characteristic of lesions in the frontal lobes. He says they may be the only symptoms present.

W. Gilman Thompson[2] has described changes in tem-

[1] Alienist and Neurologist, April, 1890.

[2] Medical News, May, 1891.

perament and alterations in the intellectual sphere occurring in three cases of tumor of the frontal lobes under his observation.

Schoenthal has also recorded a case diagnosticated as hysteria on account of the mental peculiarities and lack of self-control, in which a large tumor of the frontal lobe was found after death.

Griffith and Sheldon,[1] in reporting a case of tumor invading the median surface and base of both frontal lobes, in which mental symptoms were absent, call attention to the fact that mental symptoms occur chiefly when the cortex of the convexity of the frontal lobes is invaded; and this statement is borne out by my own collection of cases before alluded to.

The review of these cases, therefore, pointed to the conclusion that mental symptoms are likely to be produced by a tumor in the frontal region, and this fact was considered of much importance in determining the situation of the tumor in this case.

The diagnosis of the nature of the tumor was somewhat difficult. The existence of specific disease pointed to gumma, and made it seem proper to try the effect of specific treatment; he was therefore put on inunctions of mercury and increasing doses of iodide of potash, which was carried to the point of three hundred grains a day. This treatment was pursued without much apparent change in his condition. He then escaped from my observation for two months, but returned about the first of June to the Clinic.

It was then found that his headache was still severe, was still localized in the left side of the forehead. It was found that his sight was much worse, so that he was nearly blind in the right eye and could not read letters with the left eye. His hemiplegia was more marked; his face was flatter on the right side; his arm and hand more clumsy, and there was a decided dragging of the right foot. He complained that his right leg was getting stiffer all the time and that it felt dead. He said that he had recently

[1] Journal of Mental Science, 1890, 223.

been having twitching in the right leg, as often as two or three times a day. He also said that at times his hand became clenched without his power to resist it, but he denied the existence of any clonic spasms. He had had some difficulty in micturition during the past months, it being impossible for him to control his bladder perfectly, the urine flowing unexpectedly. His speech was slower, and there was a noticeable tendency to the malposition of words in sentences, which, however, he noticed himself and corrected; he would often say "no" for "yes," and *vice versa*. His mental activity was evidently much weaker than it had been five months before.

Under these circumstances it seemed evident that specific treatment had failed of effect, and he was induced to enter Roosevelt Hospital and submit to an operation.

Operation.—The operation was performed by Dr. McBurney on June 23d, 1892. Ether anæsthesia. A semi-elliptical incision was made in the scalp, outlining an area which measured about three inches in each direction, the attached base of the flap being below. The centre of the flap coincided with a point an inch and a half anterior to the fissure of Rolando, opposite the junction of its upper and middle thirds.

The tumor was believed to occupy the posterior part of the second frontal convolution, just anterior to its junction with the anterior central convolution. The hæmorrhage caused by this incision was excessive, certainly treble the usual amount, and required a large number of pressure forceps and ligatures for its control. A button of bone, one inch in diameter, was then removed with the trephine from the centre of the area exposed by turning down the flap. This opening was enlarged with rongeur forceps downward and forward until it measured two by one and three-quarter inches (Fig. 3). The dura appeared to be thickened and was unnaturally pale, but pulsation seemed normal and no bleeding was noted. Profuse hæmorrhage occurred from the veins of the diploë, and no little difficulty was met with in its management. The largest of these veins were occluded only by plugging their orifices firmly with small bits of sponge.

The dura mater was then incised near the edge of the opening in the bone and turned down as a flap. It was quite adherent to the surface of brain beneath it, which was uniformly dark in color and very vascular. At the first inspection the surface seemed to be that of a much-

congested ordinary cortical substance. It was, however, firmer in consistence than was normal, and a good-sized

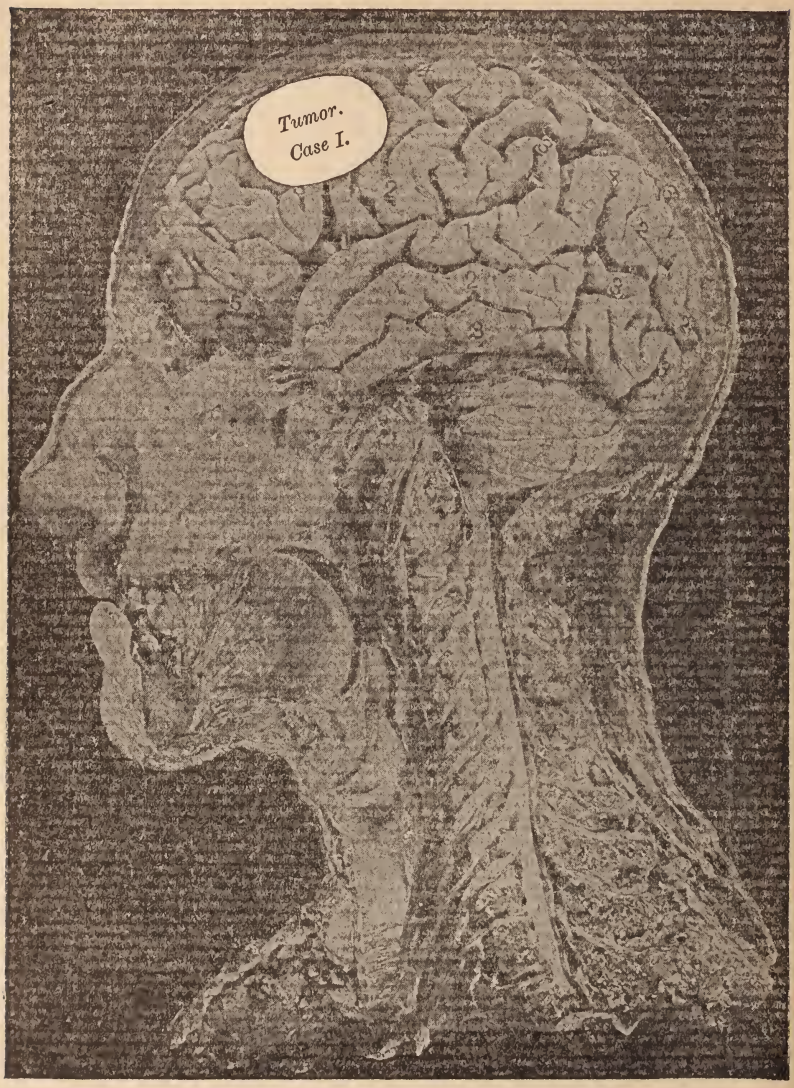

FIG. 2.—Situation of a tumor, shown in a photograph from Fraser's "Guide to Cerebral Operations." The size is in exact ratio.

section was removed with the knife; it was then clear that

the whole area exposed was tumor tissue. At the end of the section removed a distinct capsule was met with; following this with finger and blunt scissors, it was not difficult to completely enucleate the large tumor, which extended in every direction beyond the edges of the opening already made in the skull (Fig. 3). The tumor was ovoid in shape, measuring two and one-half by one and three-quarter inches (Fig. 4). It was completely enclosed by a capsule, and after its removal a large cavity

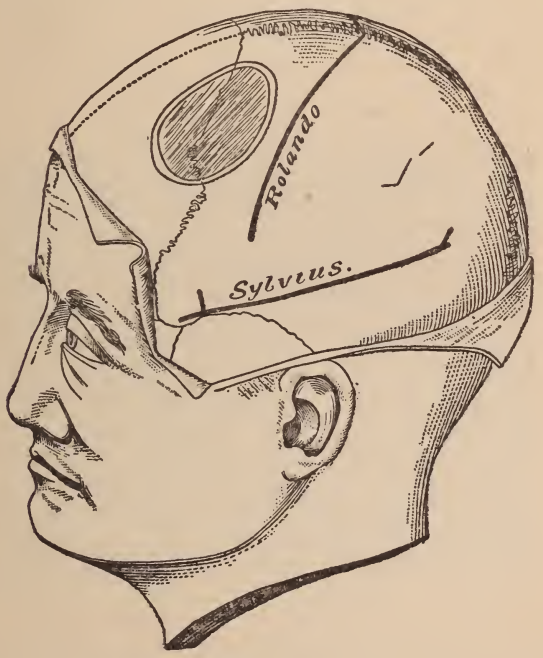

FIG. 3.

in the cortex remained. This cavity bled profusely at every point, the hæmorrhage requiring for its control complete packing with iodoform gauze. The flap of integument was partially replaced and sutured at the sides only, a large, loose antiseptic dressing being applied over all.

Loss of blood and shock produced a marked effect upon the patient's general condition before the close of the operation, and both rectal and hypodermatic stimulation was actively applied, and after the patient's removal to

bed he was given constant attention and every effort was made to improve his condition. After a large intravenous infusion of normal salt solution temporary marked improvement was noted, but the pulse soon failed again and death occurred about midnight, eight hours after operation.

The exact situation occupied by the tumor is shown in Fig. 5. It involved the posterior part of the second frontal convolution, the adjacent portion of the first frontal, and the upper half of the anterior central convolutions. The entire anterior central convolution must

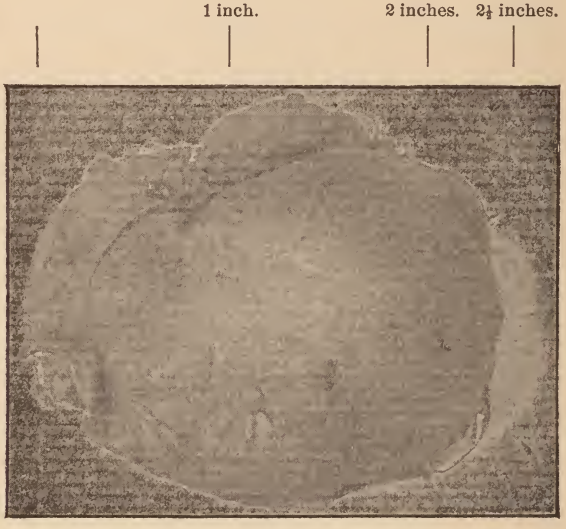

FIG. 4.—The tumor in Case I. With inch measure.

have been compressed to some degree, and indirect pressure must have been exerted upon the third frontal convolution below the tumor. The situation of the tumor corresponded, therefore, very accurately to the diagnosis made before operation, but the size of the mass was much greater than had been anticipated. After hardening in Müller's fluid and alcohol, it displaced 50 c.c. of water, weighed 4 grammes, and measured 2½ by 2 by 1¾ inches. The tumor was carefully examined by Dr. Eugene Hodenpyl, and was reported by him to be a true sarcoma, consisting of a large number of delicate blood vessels and rather large, irregular, but not branching

cells closely packed together with very little intercellular substance.

An earlier operation, when the tumor was much smaller and the vascularity of the tissues much less, would very probably have been successful. It was proposed to the patient in February, four months before it was done. The delay, which he insisted upon, was more readily submitted to because of his specific history, which induced us to give him the benefit of the doubt and to try anti-syphilitic treatment. If Horsley's dictum had been ac-

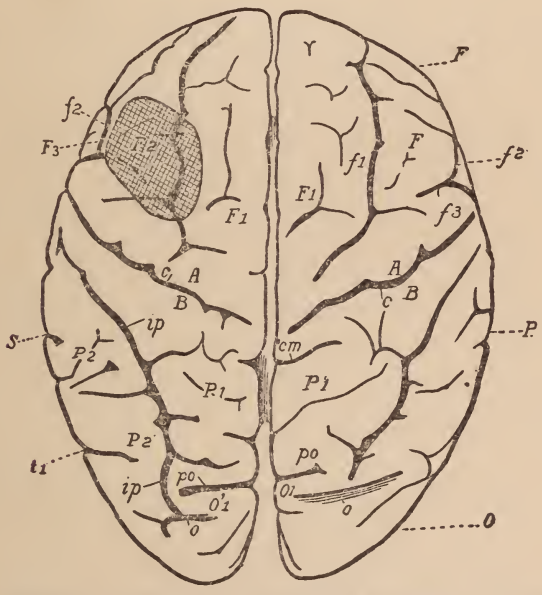

FIG. 5.—Diagram of the superior surface of the brain, to show the situation of the tumor in Case I.

cepted—namely, that gumma is not curable by medicine and should be operated for, (a dictum, however, which the experience of others in several cases does not support),—an earlier operation would perhaps have been undertaken. The size of a brain tumor has undoubtedly much to do with determining the amount of shock resulting from its removal.

This tumor was about as large as one removed by Dr. Keen and described in *The American Journal of the Medical Sciences* for November, 1888, and another operated upon

by Prof. E. Hitzig, an account of which was published in the *Berliner klinische Wochenschrift* for July 18th, 1892.

SUMMARY.—In this case the diagnosis of tumor of the brain was made from the general symptoms—headache, optic neuritis, and tenderness to percussion of the head; and from the local symptoms—mental dulness, slowness of speech, slight right hemiplegia with subjective numbness and occasional twitching in the paralyzed limbs. The situation of the tumor was determined by the slow onset of the hemiplegia, by the very marked mental symptoms, and by the location of the tenderness upon the head. Attention has already been called to the value of the mental symptoms in the localization of the tumor, and no further comment upon them is necessary. This is the first case, however, in which operative interference has been so distinctly directed by the existence of mental symptoms.

It is interesting to note that the first symptom in this case was a convulsion beginning with a turning of the head and eyes to the right. The motor centres for movements of the head and eyes have been located by Horsley at the posterior part of the second frontal convolution, and it was exactly in this region that the growth must have commenced. The absence of serious paralysis at any time—for the man was perfectly able to walk, to feed himself, or even to write, up to the day of the operation—shows that the pressure upon the motor zone of the cortex was not sufficient to arrest its function. And it is interesting to observe that this pressure, having been exerted very gradually, did not at any time induce cortical spasms, for the slight twitching of hand and leg which was complained of late in the history was not of the nature of Jacksonian epilepsy. The tumor lay upon the cortex in a capsule of its own, and did not produce any apparent pathological changes in the cortex itself, though this supposition could not be confirmed by microscopical examination. There was no evidence of softening or encephalitis. The amount of pressure or compression

which the cerebral tissue will admit without manifesting evidence either of irritation or of arrest of function was peculiarly illustrated in this case. The situation was exactly such as to have caused agraphia, had the theory of a motor centre for writing independent of the hand centre been correct, but there was no agraphia present.

Tumors of the cerebellum are not at all infrequent, and, as a rule, are easily diagnosticated. The following histories illustrate the symptomatology of the disease, and the difficulty in the way of its exact localization in the cerebellum and in the way of surgical interference for the removal of tumors of the cerebellum.

CASE II. *Fibrosarcoma of the cerebellum and pons Varolii; staggering away from the side of the tumor; operation; death.*—Male, aged thirty years; was under my observation from January, 1890, until December, 1891, when he died. He was referred to the Nervous Department of the Vanderbilt Clinic by Dr. Weeks. When first seen he was suffering from severe frontal and occipital headache; from vertigo, which was much increased by moving the head suddenly or by lying down; from tinnitus aurium; from numbness in the left side of the face and in the mouth; and from a very continuous feeling of drowsiness and dulness. These symptoms had developed gradually during the preceding three years, and within a year he had also noticed double vision and a gradually increasing blindness. His friends said that his speech had become slow and thick.

Examination showed a large, very dull, stupid man, with prominent eyes, the left one deviating outward, dilated pupils, and marked nystagmus on lateral movement of the eyes. Dr. Weeks had found well-marked choked discs and a diminution of the visual fields. There was some slowness of speech, which was accounted for by his mental dulness, there being no evidence either of aphasia, or of paralysis of the tongue. There was no disturbance of sensation, or of motion, or of reflex action, and there was no ataxia in his gait. The existence of headache, vertigo, tinnitus aurium, nystagmus, diplopia, and choked discs established the diagnosis of a cerebral tumor, but no conclusion regarding its localization could be reached. That the tumor was not a gumma was admitted, as he

denied all specific infection, yet he was put upon mercury and iodide of potash on the supposition that he might have acquired the disease without his knowledge.

During the year 1890 the symptoms continued, and gradually increased in intensity, so that by the first of October he had become quite blind, with well-marked optic atrophy ; and also deaf in the left ear, in which ear the tinnitus aurium had been intense. By this time, also, local symptoms had developed which gave an indication of the site of the tumor. There was a considerable degree of staggering in walking, with a tendency to fall forward and toward the right, and a marked tendency to turn toward the right in walking. In addition there was some weakness in his right hand, the dynamometer registering only 39, while it registered 60 in the left hand. There was no ataxia or disturbance of sensation in the limbs. There was no apparent difference in the power in the legs, but the knee jerk was exaggerated on the right side, and a slight clonus was obtained in the right foot.

The staggering was of the kind observed in cerebellar disease—a gait like that of a drunken man, without falling, but with every appearance that the balance was uncertain. The tendency to turn and to fall to the right was noticed on every occasion on which he was tested.

Diagnosis.—The staggering seemed to prove that the disease was located in the cerebellum, and the question arose whether its direction, toward the right, pointed to the side on which the tumor lay. Ross makes the statement that staggering is toward the side of the tumor. Out of eighteen cases of cerebellar tumor in which staggering toward one side was observed during life, which I have been able to find after careful search in the literature, fourteen were found to have had the tumor on the side opposite to the staggering—*i.e.*, 77 per cent—and four were found to have had the tumor on the side toward which the patient staggered. While staggering is a symptom commonly found in cases of cerebellar tumor, staggering in one particular direction is a symptom by no means frequent, being recorded in but three cases in a collection of ninety-six cases of tumors of the cerebellum in children, made by me three years ago, and in only nine

of Bernhardt's ninety cases. It is certainly more commonly observed when the disease involves one of the middle peduncles of the cerebellum, and, therefore, has been recorded in some cases of tumors upon the base involving the side of the pons Varolii. But here, also, there seems to be no absolute rule. I have recorded [1] two cases of unilateral disease of the pons with marked unilateral cranial nerve palsies in which the patients staggered toward the side of the lesion; but there are other cases on record in which the staggering was away from the lesion.

The most recent investigations upon the physiology of the cerebellum are contained in the elaborate work by Luciani, published last year.[2] In regard to the symptoms of rotation produced by the destruction of one-half of the cerebellum, he says that rotation may be of two kinds: the first due to irritation, the second due to paralysis.

1. Irritative rotation is caused by traumatism or an inflammatory condition of the peduncular fibres. It consists in rotation from the wounded toward the sound side, for in this instance the want of equilibrium is generated by a unilateral exaggeration of the cerebellar influences transmitted by the irritated peduncle.

2. Paralytic rotation is caused by the simple section of a peduncular strand. It consists in rotation from the sound toward the wounded side, because here the want of equilibrium arises from the fact that one-half of the centres in connection with the wounded peduncle suddenly lose their cerebellar influences, whilst others retain it.

In the light of these experiments, and the conclusions from them, it is evident that in those cases in man in which the patient has staggered away from the lesion the condition has been one of irritation, while in those cases in which the patient has staggered toward the lesion the condition has been one of destruction in the middle peduncle of the cerebellum. It is as yet impos-

[1] "Familiar Forms of Nervous Disease," pp. 119–123.

[2] L. Luciani: Il Cervelleto, Firenze, 1891. Deutsche Ausgabe von Dr. M. O. Fraenkel, Leipzig, 1893.

sible, however, during life to determine from the symptoms, in any individual case we are dealing with, whether there is irritation or destruction of the cerebellar peduncle, and therefore, as already observed, from the direction of the staggering any positive conclusion as to the location of the disease cannot be reached.

While the staggering in this case indicated that the cerebellum was affected, its direction to the right was not considered sufficiently diagnostic to decide absolutely the question regarding which side was involved, though it pointed strongly to the left side.

That question seemed, however, to be decided by a study of the other symptoms. The patient had complained early in the disease of pain and numbness in the left half of the face, though at no time did examination show any anæsthesia. He also had much tinnitus in the left ear, which had been followed by progressive deafness. His headache, which had at first been frontal, was later referred with much constancy to the left occipital region, and in speaking of it he habitually put his hand back of his left ear. The weakness of the right hand and the exaggeration of the spinal reflexes on the right side, taken in connection with these left cranial nerve palsies, appeared to indicate some pressure on the left side of the pons and medulla.

Thus the staggering to the right, the left cranial nerve palsies, and the right hemiplegia, all pointed to a lesion in the left side of the posterior cranial fossa.

The diagnosis was therefore made of a tumor on the left side of the cerebellum. The negative result of specific treatment indicated that it was not a gumma, and the very slow progress of the case indicated that it must be a slowly forming tumor, probably sarcoma, inasmuch as such tumors are more common than any other kind.

During the following year, from October, 1890, to November, 1891, the patient was seen occasionally, being apparently in a stationary condition. Finally he was induced to enter Roosevelt Hospital for operation. And he was then quite willing to submit, though knowing

the dangers, because his life was a burden, for he was blind and partly deaf, and suffering from severe headache, vertigo on any movement, and such exaggerated staggering that he could not go about. A careful examination in the hospital on December 1st, 1891, confirmed the existence of all the symptoms hitherto mentioned, but failed to elicit others.

Operation by Dr. McBurney on December 3d, 1891. Ether anæsthesia. A vertical, tongue-shaped flap was marked out with the knife over the left half of the occipital bone. The upper free convex border of this flap corresponded nearly to the superior curved line of this bone. The attached base was on the back of the neck, about opposite the second cervical vertebra. The incision was carried down to the periosteum, and all the coverings were removed in one flap. Experiments on the cadaver had satisfied the operator that the safest and most convenient method of entering the cerebellar fossa was by the use of the chisel and mallet. This method was adopted here, and an opening about one and a half inches in diameter was made through the thin bone, care being taken to be far enough away from the large venous sinuses. The dura mater was not diseased, but bulged very strongly through the opening in the skull in such a manner as to at once suggest great intracranial pressure. Protrusion of cerebellar tissue was still more marked after the dura had been turned back as a flap from over its surface. Otherwise, however, the surface of the cerebellum was normal in appearance, and palpation failed to give evidence of the existence of tumor. It was found to be quite easy to introduce the finger for some distance into the skull on all sides of the cerebellar hemisphere to thus examine a large part of its surface, and to distinctly palpate the lateral and vertical sinuses. But nowhere could the existence of tumor be demonstrated. So much protrusion of cerebellar tissue existed that it was necessary, in order to close the opening in the skull at all satisfactorily, to shave off the excess, which was done with the less compunction as even the gentle manipulations practised had somewhat injured the delicate surface convolutions. Hæmorrhage throughout the operation was very moderate and easily managed. The flaps of dura mater and overlying soft parts were then replaced, fastened in all deep parts with catgut, the skin wound being sewed completely with silk. A wet bichloride gauze dressing was

applied over all, and the patient removed to bed in excellent condition.

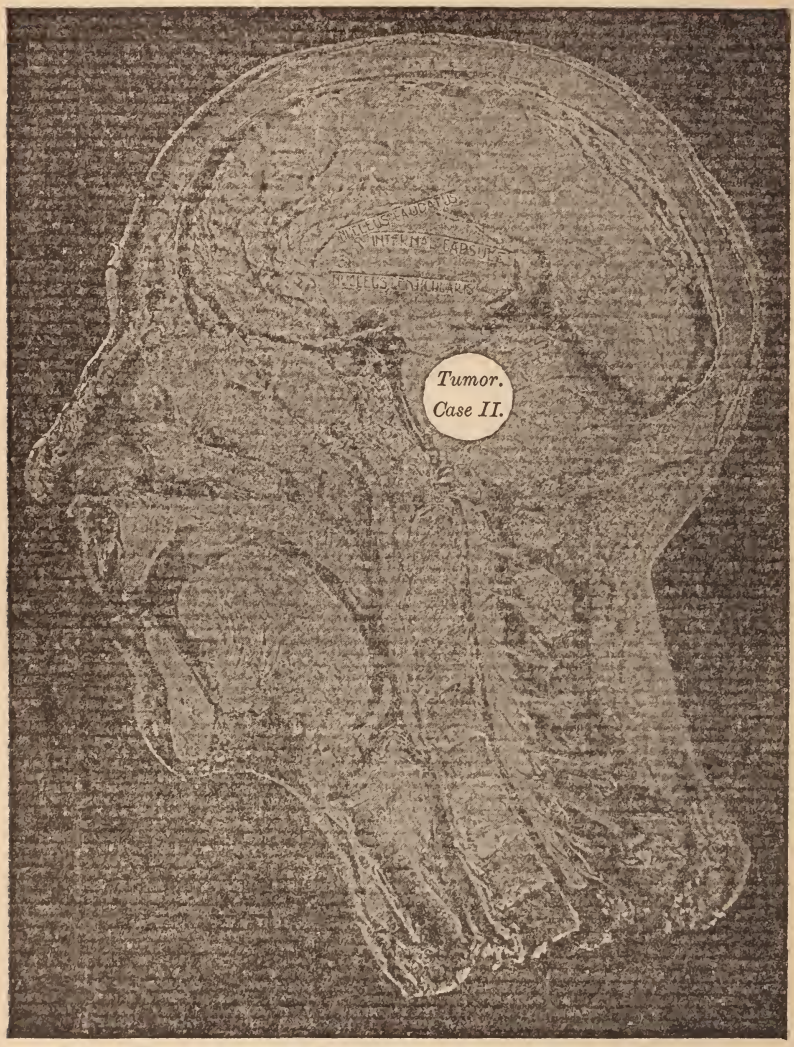

FIG. 6.—The situation of the tumor in Case II., shown on a Fraser photograph. The difficulty of removal is evident from its deep situation.

Convalescence was perfectly satisfactory, and on December 9th, six days after operation, the temperature

being 99° and the pulse 100, the dressing was changed for the first time. Primary union was found throughout the whole extent of the wound, and all sutures were removed.

During the following night the patient fell out of bed, and immediate examination revealed the presence of a large blood clot beneath the skin flap. No other injury seemed to have resulted from the fall, but at 5 P.M. on December 10th a chill occurred, followed by a temperature of 103°.

Difficulty in swallowing was then noted, and although at the end of two days the temperature fell to 99°, stupor

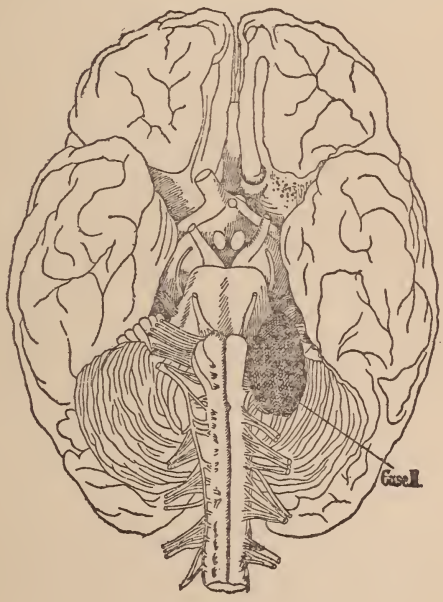

FIG. 7.

gradually increased and involuntary evacuations of rectum and bladder began. The wound remained aseptic throughout, but the stupor deepened into coma, and the patient died on December 15th with a temperature of 105°.

The *autopsy* showed the presence of a tumor, a gliosarcoma, whose limits were quite distinct from the cerebellar tissue, though it was not encapsulated (see Fig. 6).

It lay on the base, and compressed the left hemisphere of the cerebellum, and especially its anterior inferior

(ventro-cephalad) surface, and also pressed upon the left half of the pons Varolii at its lateral part. The left crust was slightly indented by the tumor, and the fifth nerve had been flattened out by it without being so pressed upon as to be degenerated. The auditory and facial nerves were also compressed by the lower part of the tumor (see Fig. 7).

The situation of the tumor was such as to have made it absolutely impossible to reach it by operation, unless, indeed, the certainty of the situation of the tumor had been so complete as to justify full section of the cerebellum. It was almost identical in situation and appearance with a tumor reported by Wollenberg in the *Arch. für Psych.*, xxi., p. 791. Its situation as related to the surface is shown in Fig. 6, which is made by drawing the tumor in one of Alexander Fraser's plates.

CASE III. *Glioma of the cerebellum; characteristic staggering; operation; death.*—A little girl of seven years of age had suffered for a year from severe headache all over the head, but chiefly in the forehead, from severe vomiting, and from gradually advancing blindness due to a progressive optic neuritis. For three months before she was seen it had been difficult for her to walk on account of a tendency to stagger and on account of dizziness, which was undoubtedly due in part to nystagmus, which was observed early in the history. The staggering was very marked, so that during the last month she could not walk without aid. She did not appear to stagger in any one direction constantly, but there was some tendency to fall backward and slightly to the left. She complained at times of earache in the right ear, but there was no evidence of cranial nerve palsy or of hemiplegia.

The diagnosis of cerebellar tumor in this case was quite evident, but the only clue to the position of the tumor was the tendency to stagger backward and to the left. It was thought probable that the tumor was in the vermiform lobe of the cerebellum, more likely upon the right than upon the left side. The absence of cranial nerve symptoms showed that it was not near the base. The operation was therefore undertaken.

Operation.—The operation was done by Dr. McBurney on December 29th, 1891. Ether narcosis. A horseshoe-shaped incision with the convexity upward was made over the right half of the occipital bone. The upper part of the incision lay a little above the superior curved

line of the bone, and the flap, which was then turned down, included all the soft tissues excepting the periosteum. The base of the flap was left attached to the upper part of the neck.

With chisel and mallet a considerable plate of bone was removed from over the centre of the cerebellar fossa, and the opening was then enlarged with rongeur forceps as much as was safe, having due regard for the venous sinuses. The dura protruded forcibly, but otherwise appeared normal. A large flap of dura was then cut and laid back, revealing only normal cerebellar convolutions. Examination of the sides and under surface of the cerebellum gave no information. A probe was then passed some distance, about one and a half inches, into the brain substance, but no abnormal resistance was encountered.

An aspirating needle introduced about one-half inch from the median line, and parallel with the base of the skull, entered a cyst from which two drachms of clear serous fluid were withdrawn.

A second introduction of the needle failed to detect the cyst, and it was deemed unwise to make further exploration. The flaps were then replaced, being stitched deeply with catgut and superficially with silk. Hæmorrhage during the operation was not troublesome, but before its close the patient showed the effects of shock. She rallied well, however, after rectal stimulation, and on the day following operation seemed about as well as on the day before it, at intervals complaining of headache only. Two days later complaint was made of pain in the ears; the wound was dressed and found to be aseptic. The temperature since operation had remained normal.

On January 4th, six days after operation, vomiting occurred repeatedly, the pulse became very weak, stimulation had no effect, and the patient died suddenly in a convulsion at midnight.

The *autopsy* revealed a large gliosarcoma, 2½ by 2 by 1 inch, which occupied the vermiform lobe of the cerebellum and extended into both hemispheres, chiefly into the right one. It lay just under the superior surface of the cerebellar cortex, but it nowhere reached the surface of the cerebellum. It projected downward, compressing the fourth ventricle. Its consistence was about that of the cerebellum, and in its centre was a cyst which had been evacuated by the aspiration.

Certain comments upon these two cases seem warranted. First: *As to the diagnosis.* It is easy to diagnosticate cerebellar disease—headache, vomiting, vertigo, blindness, and staggering being the symptoms almost uniformly found. It is as yet almost impossible to locate the disease accurately within the cerebellum. In 16 cases out of 20, that is, in four-fifths of all cases recorded, the staggering, when uniformly toward one side, has been away from the lesion; yet in one-fifth of the cases, and in several cases of disease in the pons Varolii involving the basal surface of the cerebellum, the staggering has been toward the lesion. From the direction of the staggering alone, therefore, it is impossible to determine exactly the side on which the disease is. The symptoms of the greatest importance in the location of cerebellar disease appear to be those arising from the involvement of the cranial nerves upon the base. The disease commonly lies upon the side upon which these cranial nerves are affected. Such symptoms are usually due to direct pressure upon nerves on the base. They are sometimes due to displacement of the entire cerebellar axis by indirect pressure from a distance. If the cranial nerve symptoms are on the side away from which the patient staggers, then there appears to be a double probability that the tumor is on the side on which they appear. This was the condition in my first case, and the autopsy proved the truth of the probable diagnosis. The complete absence of cranial nerve palsies indicates that the tumor is not near the base. As staggering only occurs when the vermiform (middle) lobe is invaded by the tumor, its absence would show that the tumor did not lie near the median line.

The localization of disease in the cerebellum being therefore a matter which cannot be positively determined from the symptoms, any effort at its removal must necessarily be tentative. There are many cases reported of tumor of the cerebellum in which it is perfectly evident that an operation would have been successful. In these cases the tumor lay in the posterior fossa directly beneath

the bone, was not adherent to the cerebellum, and could have been easily removed. An exploratory operation, therefore, would have succeeded in these cases. In a disease which is necessarily hopeless any means certainly should be used which may offer a possibility of relief. Therefore, in these cases of cerebellar tumor, even though the diagnosis be somewhat uncertain, an exploratory operation seems to be advisable.

Secondly: *The dangers and difficulties attendant upon an operation for cerebellar tumors* are much greater than for tumors of the cerebral hemispheres. The muscular tissues which have to be divided in order to lay bare the occipital bone are thick and richly supplied with blood vessels, therefore hæmorrhage is free and not easily controlled. After the muscular tissues have been cleared away and the bone laid bare, the bony surface open to operation is exceedingly limited, the sinuses leaving but a small portion of the bone open to the trephine. The part accessible is a triangle, rarely more than two inches in its greatest diameter, on each side of the median line. The opening through the occipital bone gives access to a portion of the posterior inferior surface only of the cerebellum; its anterior inferior surface and the greater part of its superior surface are wholly inaccessible. The fossa in which the cerebellum lies is small and the organ is held down into it tightly by the tentorium; it cannot therefore be freely pressed aside by the exploring finger even in the normal state, and when the intracranial contents are increased by a tumor the pressure in the fossa is so great that the opening of the skull is apt to be followed, as in my first case, by an immediate bulging of the cerebellar tissue, which prevents exploration. It is true that the cutting off of such tissue appears to cause no serious symptoms, yet it cannot be absolutely harmless. The difficulties of the operation are therefore not small.

The records of cases operated upon for cerebellar tumor are by no means encouraging. Up to the present date eleven such cases have been recorded. These are as follows:

CASE I.[1]—Male, aged seven; suffered in April, 1886, from headache, chiefly frontal, and vomiting; then gradual failure of sight developed, and in July he was nearly blind. In July paralysis of the right sixth nerve was noticed and the eyes were turned to the left. Optic neuritis was then found in both eyes. At this time the gait became affected; he staggered, and tended to fall backward and to the left. His mind was clear. In August the headache and vomiting became severe, he could not stand, and the head was retracted. In October he had become totally blind and nystagmus had appeared. There was loss of knee jerk on the right side and general weakness, with great emaciation.

He was then operated upon by Mr. May. The cerebellum was exposed on the right side of the median line, and appeared to be healthy, but bulged and was felt to be hard at one spot. After incision at this spot a tumor was felt one inch below the surface. This was dug out with the handle of a spoon. It was larger than a pigeon's egg, hard on the surface, caseating at its centre. Hæmorrhage was slight, but the child died of shock a few hours afterward.

CASE II.[2]—Male, aged eighteen; had suffered from headache, vomiting, optic neuritis, increasing weakness of all his limbs, especially of the left arm and leg, vertigo, and typical staggering gait of cerebellar disease. He also had epileptoid attacks, with turning of the head and eyes to the right. His mental state was good, but he was much emaciated and had been in bed a year. Dr. Bastian made the diagnosis of a tumor involving the right lobe of the cerebellum, and, as a last resort, Mr. Horsley attempted its removal. After trephining, a tubercular tumor was found in the right lobe of the cerebellum and removed. It weighed seven drachms. The patient sank gradually and died nineteen hours after the operation.

CASE III.[3]—Female, aged twelve; complained of headache and vomiting for eighteen months, then for nine months of increasing weakness of the right arm and leg, and then for three months of severe frontal pain over the right eye. During the last month she had noticed an inability to turn the eyes to either side, and both double and dim vision. On admission to the hospital the left

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[1] Bennett May: Lancet, April 16th, 1887, i. 768.

[2] Horsley: British Medical Journal, 1887, i. 865.

[3] Suckling: Lancet, 1887, ii. 656.

pupil was found to be larger than the right; there was nystagmus on any attempt at movement of the eyes; there was loss of conjugate motion of eyes to the right, and impairment of motion to the left; there was great impairment of vision, with double optic neuritis. Weakness of right extremities with diminution of knee jerk, staggering gait with a tendency to stagger to the right and to fall forward, and marked tremor of the right arm on any motion, were found. There was a slight paralysis of the left side of the face, but the tongue deviated to the right. Headache, vertigo, and vomiting were very severe and constant. The diagnosis of cerebellar tumor was made, and an operation was considered advisable.

The occipital bone was trephined over the left side and the cerebellum exposed. It bulged out of the wound, and its tissue appeared darker and softer than normal. A part of the cerebellar substance was cut away and the wound was dressed. The patient went into a state of collapse and died of exhaustion in forty-eight hours. The autopsy showed that the glioma had occupied the entire left lobe of the cerebellum and had invaded the middle lobe also.

CASE IV.[1]—Male, aged eighteen; had suffered from headache, vomiting, vertigo, and cerebellar ataxia, with reeling to the right. His eyes were very prominent and his vision almost lost on account of advancing optic neuritis. His head and neck were swollen and retracted. He was deaf in the left ear and had lost his sense of smell. His pupils were dilated; he had convulsions in the right arm and leg. He had lost control of the sphincters. He was trephined on the 12th of February, 1889, over the left hemisphere of the cerebellum. An hydatid cyst, four by three inches in size, lying beneath the tentorium, was found and evacuated. He recovered from the operation, but remained in a condition of blindness and deafness.

CASE V.[2]—Male, aged twenty-eight; suffered from headache, vomiting and vertigo, and staggering gait; optic neuritis had produced blindness, and he was also deaf. There was left facial paralysis and awkwardness of the left side. He was trephined April 20th, 1888, over the left lobe of the cerebellum. A solid nodule was found

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[1] Maunsell: New Zealand Medical Journal, 1889, ii., 151.

[2] Maudsley and Fitzgerald: London Medical Recorder, June, 1890. Quoted by Knapp.

fixed to the bone, but this could not be removed. Much cerebellar tissue escaped from the wound. The patient recovered from the operation, but the blindness and deafness persisted.

CASE VI.[1]—Male, aged thirteen; after a blow on the right side of the forehead, suffered from headache, vomiting, uncertain gait, optic atrophy, and convulsions beginning with head and eyes turning to the right. He was trephined over the right lobe of the cerebellum. Glioma of the middle lobe was discovered. Much serous fluid was evacuated. The glioma could not be wholly removed and the patient died of shock.

CASE VII.[2]—Female, aged six; suffered from vomiting, vertigo, headache, optic neuritis, cerebellar gait, partial paralysis of the right leg, increasing later to total paralysis of all the limbs, with increase of the reflexes. The operation was undertaken for the removal of the tumor of the cerebellum, but on laying bare the bone a hole was found, over the torcular, opening into the sinus, and hæmorrhage from this could not be stopped. The tumor was found at the autopsy.

CASE VIII.[3]—Child, aged two; general symptoms of brain tumor, optic neuritis, exophthalmus, convulsions. Exploratory trephining over the cerebellum. Failure to find the tumor. Death four days later. Tubercle the size of an egg found in the left lobe of the cerebellum.

CASE IX.[4]—Male, aged twenty-eight; suffered from headache, vomiting, optic neuritis, blindness, deafness, tenderness in the right temporal region, paræsthesia of the mouth and hands. He was trephined by Dr. Bradford over the tender spot on the right temple. Nothing was found. A left hemiplegia slowly developed after the operation, and two months later he died. The tumor was found in the left lateral lobe of the cerebellum.

CASE X.[5]—Male, aged thirty-nine; suffered for eight months prior to his operation from headache, vertigo, vomiting, and a staggering gait, with tendency to fall backward. He also developed optic atrophy and great mental dulness.

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[1] Springthorp and Fitzgerald: Australian Medical Journal, November 15th, 1890.

[2] Bullard and Bradford: Boston Med. and Surg. Journ., April 30th, 1891.

[3] Lampiasi: Wien. med. Wochenschr., May 19th, 1889.

[4] Stewart: Pittsburg Medical Review, November, 1892.

[5] Italian Congress, Surgery, 1892: Annals of Surgery, December, 1892.

Finally he became unable to stand, or even to sit up in bed, on account of his tendency to fall backward. He also had tetanoid spasms of the neck and arms, which were worse upon the right side.

There were no cranial nerve palsies. The diagnosis being made of a tumor of the cerebellum, Dr. Stewart trephined over the left hemisphere. The cerebellum bulged greatly, but appeared to be normal, and a puncture with a hypodermatic needle failed to reveal the existence of a tumor or of fluid. The wound was closed.

The autopsy, two days later, showed the existence of a large gumma in the right hemisphere of the cerebellum.

CASE XI.[1]—Potempski reports a case of supposed cerebellar tumor with characteristic symptoms in which two attempts at removal were made. At the first attempt the cerebellum was trephined on the right side; a large amount of fluid was evacuated, but palpation failed to reveal the tumor. After the wound had healed a second attempt was made by trephining over the left side; again a considerable amount of fluid escaped, and it was thought that a hard nodule could be felt in the region of the vermiform lobe, but this could not be removed. At the time of the report the second wound had healed, but the patient was still suffering from his original symptoms.

Our own two cases bring the total number of operations for cerebellar tumors up to thirteen.

In six cases the tumor was not found at the operation, and the patient died. In one case the tumor was not found, but the patient lived up to the time of the report. In two cases the tumor was removed but the patients died. In two cases it was found but could not be removed, and the patients died. In one case it was found, not removed, but the patient lived. In but one case was the tumor successfully removed.

The percentage of deaths after cerebellar operations thus far has been 77 per cent, while that after cerebral operations has been 51 per cent.

CONCLUSION.—If we combine the results of the cerebral

[1] Knapp and Bradford: Journ. of Mental and Nervous Diseases, January, 1892.

operations with those of the cerebellar operations, we find that the total number of patients operated upon for intracranial tumors has been eighty-seven, and that of these forty, or 46 per cent, have proved successful, the patients recovering.

Experience will doubtless result in a more favorable outcome as time goes on, and neither neurologists nor surgeons should be discouraged in their attempts to relieve a formerly fatal disease.

DISCUSSION.

DR. R. H. DERBY said that in cases of tumor of the brain an optic neuritis is found in more than fifty per cent of all cases. In every case where the suspicion of cerebral disease exists the fundus oculi should be examined with the ophthalmoscope. Not alone optic neuritis is found in cases of brain tumor, but primary nerve atrophy is met with. Pressure upon the chiasma or the tractus opticus may and often does lead to an atrophy of the nerve fibres.

DR. L. C. GRAY complimented the authors on their frank report of cases not altogether successful, and he hoped that all neurologists and surgeons would imitate their example, in order that all the facts might be had on which to base an opinion as to the diagnosis and propriety of an operation in these puzzling cases. He had himself several cases yet to add to the unsuccessful list, as had also Dr. Starr. Our present means of localization applied to a few and limited areas of the brain, and even in those instances one was unable, as a rule, to say whether the tumor would be accessible or be removable until the skull had been opened. There was still another difficulty not alluded to by the readers—namely, possibility of multiple tumors, which was illustrated by several of his cases. He was glad the old-fashioned method of making a mere peephole with the trephine was no longer practised. An early operation was urged, for he had again and again seen removal of a tumor fail because put off until too late. As to optic neuritis, while it developed in a large proportion of cases, yet it was not, as a rule, recognized until other symptoms had established the diagnosis. Even if the tumor were not found, an operation often resulted in much benefit by relieving pressure, as had been shown

in some of his cases, and he had twice seen life prolonged for three or four months by this means.

Dr. H. Knapp said that he had had the privilege of witnessing the operation on the first patient. He only rose to express his admiration of the superior diagnostic penetration and the consummate surgical skill with which the case had been handled. The localization of the tumor had been as precise and the opening of the cranium as direct as if the skull had been of glass through which both the neurologist and the surgeon had been able to see the pseudoplasm.

Dr. C. L. Dana gave statistics of twenty-nine cases of brain tumor observed in private practice and at Bellevue Hospital the past five years, all of which he had examined except ten. Twelve were cases of persons who had died at Bellevue and autopsies were made there. Three other cases had died and autopsies were made. The remaining fourteen had been studied clinically and were either still living or had been lost sight of. Out of the twenty-nine, two were operated upon with fatal result. These were both tumors of the frontal lobes. Three others might have been located and operated upon with a good chance of success. These were all tumors of the cerebral convolutions. Thus ten per cent of the twenty-nine cases might have been operated upon with a fair show of success. Successful operations, however, might still leave the patient paralyzed, or in as bad a condition as before, if not worse.

Dr. B. F. Curtis said he had operated on but one case for brain tumor, tubercular, which had already been reported. It was in the temporal region, was successful, the patient was improved, but recurrence took place after some weeks and caused speedy death. In operating morphine-ether anæsthesia gave a remarkably quiet pulse and perhaps accounted for the unusually small amount of hæmorrhage.

Dr. J. E. Weeks said that the two cases of cerebellar tumor had come under his observation and that he had referred them to Drs. McBurney and Starr. He had been struck by the complaint of severe headache and nausea, with temporary relief by vomiting, also by the continued fever, and inquired of Dr. Starr the significance of these. He then spoke of the significance of optic neuritis in tumors of the brain, it being present in about four-fifths of

all cases. After reviewing the theories offered for its explanation, Dr. Weeks said the papillitis was probably due to neuritis conducted along the trunk of the nerve by continuity of tissue until it reached the eye.

DR. H. D. NOYES said it was true that optic neuritis was likely to come as rather a late symptom in brain tumor, nor could one tell by its appearances what was the nature of the inflammation or process in the brain which caused it, much less determine thereby the location of the cerebral trouble. Change in the field of vision, however, due to direct or indirect encroachment upon the visual cortex, possessed some localizing value. When this takes the form of hemianopsia attention must be given to the visual paths, either the peripheral or central portion. To appreciate this symptom sometimes much pains must be taken to fix the patient's attention, and partial defects in the visual fields especially require exact study; when discovered they sometimes afford valuable aid. Defects of color perception are also to be investigated in chronic cases, for they sometimes are to be attributed to cerebral lesions.

DR. E. D. FISHER thought the direction of staggering of little value in deciding on which side the cerebellar tumor was located. He had recently made an autopsy in a case of sudden death with hemiplegia, which had been diagnosticated as hæmorrhage of the brain. A tumor of the cerebellar lobes was found, pressing somewhat on the middle lobe, and an operation would probably have proven successful, but there had been no localizing symptoms. While the outlook for operation on brain tumors in general was bad, yet he believed it should be undertaken, since without it the patient would die.

The authors closed the discussion. Replying to Dr. Weeks, DR. STARR said he had seldom found fever in brain tumors, and did not think it of diagnostic importance. Headache was important in pointing to brain trouble, but its location gave little indication of the seat of lesion. In syphilis it was largely nocturnal. He agreed with Dr. Noyes as to the value of hemianopsia as a local symptom of brain disease.

Stated Meeting, January 19th, 1893.

THE GROWTH OF THE ACADEMY.

VALEDICTORY ADDRESS.

By ALFRED L. LOOMIS, M.D., LL.D.,

THE RETIRING PRESIDENT.

Fellows of the New York Academy of Medicine :

BEFORE I pass to my distinguished successor the insignia of office with which you honored me four years ago, it seems fitting that I should review in brief the work of these years, that we may the better appreciate the rapidly increasing breadth of our fellowship, and recognize that privilege and obligation are twin sisters in their growth.

The story told in dates and figures alone might be cold and formal, but the ratio between 640 and 270 bristles with interest when each unit means a Fellow, and we recognize that the new members received in four years equal more than thirty-three per cent of our entire membership so short a time ago. And interest passes to pride when among the new faces we see so many of the older members of the profession, whose honorable positions and intellectual work have brought us both strength and renown. Nor is our pride without hope for the future. Among the younger members who now share our fellowship are found many of the most active workers in the profession, not a few of whom have already gained honorable distinction for work done. Every line of our growth is marked by equally eloquent figures. The 7,000 new volumes and the 2,000 new pamphlets which already crowd our present shelves to overflowing, and have raised the catalogue of our library to 27,000 volumes and 8,500 pamphlets, represent but a slightly lower per cent of increase. The

breadth of privilege which these figures indicate cannot be made more clear, nor is it possible to obscure the twin obligation. Men and books are valuable only when combined with mental processes. How faithfully we have met this obligation is shown in the fact that the number availing themselves of the privilege of our library has increased in three years three hundred and fifty per cent, while men and means have increased one-third. The work done in our library has increased three and one-third times. These figures gladden our hearts, for they indicate that our fellowship is doing ten times more brainwork than it did four years ago, its intellectual status is being elevated, and a deeper enthusiasm and stronger purpose is giving more certain relief and comfort to suffering humanity.

It may not be uninteresting to recall the more material side of our history in these harvest years.

In 1888 we held a clear title to the Academy building in 31st street, which was valued at $60,000, and our building fund had reached $96,000. Under the stimulus of Dr. Jacobi's enthusiasm there had been many discussions in both Academy and Council regarding the immediate necessity of a new Academy building, which finally crystallized at the regular meeting of October 18th, 1888, into the unanimous passage of the resolution "that in the opinion of the Academy it is now expedient to take active steps toward the purchase of a site and the erection of a new Academy building." The Trustees and Council were instructed to carry out the intents of this resolution. Owing to unexpected complications it was not until March 7th, 1889, that the Council reported to the Academy the execution of a contract for the purchase of a site on West 43d street, at a cost of $90,000. Formal authority to draw upon such funds of the Academy as could be legally used for that purpose was conferred upon the Trustees by vote of the Academy at the meeting of April 14th, 1889, when a special Building Committee, composed of Drs. Jacobi, Herrick, Peters, Castle, and Loomis, was appointed and entrusted with

all matters of detail. Two months were spent in consideration of plans and designs, and on June 4th, 1889, the Academy accepted unanimously those of Mr. Robertson, which the Building Committee had submitted as having their approval, and authorized the Committee to make contracts for the erection of a building in accordance with these plans, and under the supervision of Mr. Robertson as architect. These instructions were obeyed with the least possible delay, and on October 2d, 1889, less than one year from the date when the Academy assumed a definite position on this question, the corner stone of this building was laid with appropriate ceremonies. At the end of another year your Building Committee formally transferred this completed building to the Board of Trustees.

The anniversary meeting of November 20th, 1890, was made memorable by the dedication of the most complete and appropriate building of its kind in the country, and the formal opening of its library to the profession and the public. It was an occasion which will be long remembered, not alone by those present, but by all who appreciate the full significance of the event; when distinguished members of our profession from neighboring cities, and eminent citizens of our own city, who had given generous proofs of their sympathy, united with us in celebrating the consummation of our long-cherished hopes.

Never was there more beautiful illustration of the truth that, to earnest men, the end is but a new beginning, than in the spirit of all that was said or done on that occasion. Our anniversary orator looked backward only to see in the zeal which had attended our growth from the first the promise that our Academy was to go forward in the future to enlarge in generous measure her chosen work and consummate her functions, sustained by a common impulse among her Fellows to work for work's sake.

That eloquent champion of our library, your honored ex-President, Dr. Jacobi, saw in this building no home

for lotus-eaters, but the visible vestibule for a new epoch in medical learning and achievement. His strong words found faithful echo in the trembling accents of our honored, our beloved Barker. With the dawn of another life already tinging his horizon, his prophetic promise to this Academy of a new era, so full of grander things that none would venture to cast a horoscope of its future, was spoken in tones that almost made real the spirit of "now, Lord, lettest thou thy servant depart in peace"; nor were such conceptions distorted images of blind affection.

In the communication sent us by our noble and generous friend, D. Willis James, were these words, which should be repeated to every large-hearted citizen of New York: "Look at the magnificent work the medical profession has done and is doing. Remembering all they have accomplished for suffering humanity, let us, as citizens of New York, see to it that in our midst the means are amply and promptly supplied for the most scientific research in all departments of learning, but especially in medical science."

That full appreciation of our profession is too often lacking when most expected, must be confessed with sorrow, for Weir Mitchell told us that, although the medical guild possessed a high code of morals which was old before Christianity was born, a code based on honor, loving brotherhood, and the largest charity, even when made practical in self-sacrifice, it has never been fully appreciated even by its recipients, else would they be more eagerly generous in their assistance in building halls and furnishing libraries.

The old but ever new spirit of our profession, which has borne it onward and upward regardless of rewards, was most beautifully portrayed by our loved and honored brother, Oliver Wendell Holmes, who wrote in his greeting for that night: "An Academy which fulfils its highest function is a true working body. It deals with loving subjects. It handles unsettled questions. It sets tasks for its members and furnishes, so far as it can, the

appliances required for their accomplishment. It offers rewards for meritorious performances and sits in judgment on the aspirants for distinction. It furnishes the nearest approach we can expect to a fixed standard of excellence by which the work of new hands and the new work of old hands can be judged. It is a barrier, a breakwater against the rush of false pretensions which are constantly attempting to find their way into public confidence. Academies have been too often thought of as places of honorable retirement and dignity and ease-nooks, where emeritus professors and effete men of letters, once cocks of the walk, could sit in quiet roosts while the fighting, the clucking, and the crowing were going on beneath them." With such words of cheer and sympathy, of promise and prophecy, of warning and advice, amid the gracious smiles of approving friends, this building was dedicated a little more than two years ago. Experience has shown it in every way suited to our wants and work, and from the day of its dedication until now not only has prosperity attended us, but a broader and more tolerant spirit has been developed in our membership. The links which bind together the senior and junior members of our fellowship have grown stronger; jealousies and intolerance have given place to liberality and kindness, and the Academy has become the centre of the best thought and the best spirit of the entire profession. The young enthusiast and the aged conservative are crossing swords with that spirit of modest recognition of truth in which one scientific worker is always willing to meet his fellow. We are to-day exerting a greater influence on public thought and action than ever before. We are being more and more appealed to in the legislative and economic work of the commonwealth. The public health and safety of our citizens is being more and more committed to our hands. Last summer, when our city was threatened by the invasion of a pestilence, the whole community looked to us for protection, and through the governing body of our nation's com-

merce appealed to us for advice and guidance. Do you realize, gentlemen, how large a feeling of safety comes to the public on account of the presence of the Academy in the midst of our city? Let us act wisely and unselfishly in all matters pertaining to the public good.

In our first appeal made to the profession and public to erect a new and more commodious Academy building, it was stated that our most pressing need was a suitable place for our library.

Expressive as they are, the figures which I have given you of the library are but lifeless proofs of the eloquent words spoken by Dr. Jacobi at the laying of the corner stone, and with increased fervor at the opening of this building. They were prophecies which are rapidly being fulfilled. The real influence of our library is in the intellectual development of our fellowship, which cannot be measured by figures, but must show itself in the more exhaustive and accurate work done by our members.

But it must be remembered that the cessation of growth is the beginning of degeneration. If we are to go forward our library must not fall behind. Its endowment fund is now only $25,000. It should be increased at once to $100,000. With no less a sum can we keep pace with the present tropical growth of medical science.

That some of the large-minded men with ample means will soon discover their privilege is no longer a matter of hope but of certainty, for money in no other place can exert such a constantly widening influence for good as the increase of our library endowment fund to the sum of $100,000.

There are not a few in our midst to whom the entire sum seems small, and I would say to those who would share this honor and receive a monument of gratitude more enduring than marble, Do not delay lest the opportunity be lost. The bronze in yonder tablet was bought for a few dollars, and on its face it simply says, "This man built a hall." But let me say to those who hear or read these words that this man's name will fall from

reverent lips in tones of love and honor when all marble piles are ground to dust.

With our library fund raised to what it must be, our strength would be complete, for all the departments of medicine and surgery are now represented in our Sections, so that every Fellow can find a place among congenial workers for making public the results of his study under the sifting but kindly criticism of experts.

The plan by which our general meetings of the Academy each month are placed in charge of the Sections in rotation, thus representing to the Academy the special work being done in them, has not only added interest to the Academy meetings, but has developed a spirit of generous emulation under which all the work has become more systematic, and Sections that four years ago were indifferently attended, doing desultory work, have now become active and efficient parts of our organization. The fear that increase in the number of Sections would diminish interest in the general meetings has proven groundless, for the attendance at both the Academy and the Section meetings has increased rather than decreased.

These evidences of a broader and deeper scientific spirit give, doubtless, promise of better things to come. When we appreciate that no hampering debts can distract us from our higher purpose, we enter upon our future not only free from debt, but full owners of a building worth $380,000, with furnishings representing $30,000, a growing library valued at $80,000, and invested funds, over and above the $10,000 bonds still uncancelled, to the amount of $32,000, making a total of more than half a million. With such a financial status and such a fellowship the question comes to us to-night, What is the mission of the New York Academy of Medicine?

At the laying of the corner stone of this building Mr. Cleveland said that "the nobility and sacred character of this mission will never lose its interest while humanity is touched with human woe; while self-sacrifice receives the homage of Christian hearts; while the suffering and sor-

rows of our fellow-men start the tears of pity, nor while their alleviation brings comfort and satisfaction to the soul of sympathy." This broad and clear conception of our mission is from a layman's standpoint, but represents work done, not doing. For us the question is one of ways and means, of obligations that are individual and collective. To the individual Fellow we offer opportunities and means for a richer intellectual development and social culture, and justly demand that he use them for his own and our common good. No Fellow has a right to ignore this obligation of fellowship, for the Academy stands before the public for what its individual Fellows may be. For us, obligations and rewards are reciprocal. Whoever shares in honors won by others is bound to make return to the best of his ability.

To every one the first duty is a recognized connection with one or more of our Sections, a connection which imposes the obligation to attend its meetings when possible, to pursue studies and investigations that shall be worthy the attention of the Section, to give encouragement and support to fellow-members by serious and earnest consideration of the subjects they may present.

As an organized body we have no more imperative duty, or one requiring greater wisdom, patience, perseverance, and courage, than in organizing and harmonizing medical work. It is worse than folly to shut our eyes to the fact that so large a per cent of medical work is done to fill time as well as space as to become a matter of jest and sarcasm even among ourselves. It is worse than cowardice to confess the impossibility of doing enough good work to satisfy the demands of all our societies and then make no effort to correct the wrong.

If I may still be permitted to offer one word of advice from this presidential chair, it would be this: That the foremost duty of this Academy to-day, and the one offering in its completion the largest and most valuable returns to us and the profession, is the consolidation and concentration of the medical societies of this city. I do not say

this because the many other societies are not doing good work—many of them are doing some of the best that is done—but I do say it because they and we are doing some bad work; because we need their good work as they need ours, and neither needs bad work. In these statements I do not, of course, include the County Society. It occupies a unique position, accomplishes results that we could not accomplish, and possesses functions that cannot be delegated. It and the Academy are complementary bodies. With this exception, then, I am convinced that consolidation of the many small societies with the Academy would be productive of the greatest good to all concerned; that it would do more to conserve energies, to eliminate work that in being bad is injurious as well as valueless, to establish higher ideals, to return more generous rewards for work done, and finally to develop a stronger spirit of common brotherhood, than any step which has been taken by the profession. I am unable to see any valid objections to such a union, unless a chartered society holds property accepted under specific restrictions.

The predominance of the social element, which has been advanced as an objection, appears to me a most cogent reason for taking such a step. The introduction of the social element to the regular monthly meetings of the Academy during the past two years, which was made possible by the liberality of some of our Fellows, has demonstrated that the value of the scientific work is enhanced rather than lessened, the discussion of the evening being frequently continued in the groups gathered about the tables. There can certainly be no objection to the addition of the social element even to the Section meetings, whenever the members desire it, and thus fulfil all the claims of the social medical societies.

I cannot leave this chair of office without a word of tribute to the Fellows whom death has taken from us during the past four years; among them are three to whom this Academy will always owe a special debt of gratitude

and respect, Dubois, Barker, Leaming—names which we cannot follow with the hackneyed phrase, "We mourn their loss." As men and friends the parting brought to each of us in varying measure the aching heart and weary sense of hopeless loss. But to this Academy they can never be lost. The story of their loyalty and devotion, their watchful care and self-sacrifice, their wise counsels and liberal support, must continue to exert an influence on every Fellow who values an honorable name. We can pay them no loftier tribute of respect and appreciation than to say "we do not miss them," for that is the test of a rounded life. We restore the borrowed garments to the earth from whence they came, and call them the man. Why do we thus deceive ourselves? That faithful canvas shows but the house where Barker tarried for a time. Those hands with woman's tender touch were only tools which did his bidding; and when the recreant voice gave halting service and laggard obedience, who thought to say his wit had dulled, his learning failed, his foresight dimmed, his counsel erred, his love grown cold?

These men did all that man can do. They did their part in life with faithful zeal until their powers gave way, and with that wisdom which never dwells in sordid minds they died, not to leave their mental wealth for legacies, but day by day transferred it to men with fresher strength, and so gave over, not gave up, their work, content in the knowledge that it was not to cease but grow larger year by year; and when at last their summons came, the best of all they had—the essence of the man, his quality—they left with us.

Why is it that we will not see the truth within this mystery of death? We whine in greedy selfishness, "No man will miss me when I die," and will not see the reason that stares us in the face—the useless man receives his share of being missed when living, and cannot ask for more; the man whose worth is sterling cannot be missed: the force, that is, the man, goes on. As we turn our faces to the front, then, let us not forget these present, if

unseen, Fellows, but, made stronger in their influence, let us press forward with unfaltering courage and determination to make and share such measure of worth that we too shall not be missed.

Fellows of the Academy: Four years ago when you honored me with the presidency of this Academy I entered upon the duties of the office with a feeling of self-distrust; two years ago, when you unanimously elected me to a second term, I felt grateful for the renewed expression of your confidence; but when a Fellow at the close of my second term nominated me for a third term, a feeling of pride came to me, for no greater honor could have been conferred upon me. In declining the nomination I realized that the time had come when the interests of the Academy would be best served by new energies, stimulated by a fresh enthusiasm. For all your patient courtesy and the many evidences of respect and confidence shown me during these years I give you my sincere thanks, with the feeling that another and the most interesting chapter of life's labor and waiting is ended.

The last and most pleasing duty now left to me is to introduce my successor, Dr. D. B. St. John Roosa, who brings to the high office to which you have elected him great executive ability. His scientific attainments have already placed his name high in the list of our country's honored ones. Under all circumstances he has the courage of his convictions and believes that "the blood of the martyrs is the seed of the church." The energy and ability which Dr. Roosa has shown as president of other societies bear promise to the Academy of great successes in the near future.

THE HIGH AIMS OF THE ACADEMY.

INAUGURAL ADDRESS.

By D. B. ST. JOHN ROOSA, M.D., LL.D.,

PRESIDENT-ELECT OF THE NEW YORK ACADEMY OF MEDICINE.

Gentlemen, Fellows of the Academy of Medicine:

I AM deeply sensible of the honor conferred by an election to the Presidency of this Academy. I thank you sincerely for it. It will be my earnest endeavor to prove myself not entirely unworthy of the confidence displayed in me by the profession of this great city. The administration of the Academy, from simple beginnings, in small and almost unworthy quarters, has come to be an affair of more importance than it was in the days of its foundation. We are now in an abode worthy of the profession to which we belong, and there are nearly eight hundred members who may participate in its scientific and executive work. The growth of the Academy, like that of a good tree carefully planted by sturdy hands in a fertile soil, has been gradual but sure. A long line of illustrious names marks those who have thus far guided the work of this society. In the last two terms especially—those of Jacobi and Loomis—it has been the good fortune of the profession to see the realization, to a considerable extent, of the ideas of the founders. There have been great material advances in the partial endowment of the Academy, in the complete organization of the Sections, in the creation of the library, and, finally, in the erection of the new building. Indeed, the Sections have been so increased in number and so excited to new activity as to cause a fear, occasionally expressed, that the general meetings of the Academy may ultimately be

neglected. If the Sections are exciting more interest than the general meetings, it is probably only an unsymmetrical expansion of one part of the great structure, which, in time, will be compensated for by additions on other sides. To-night, at least, we may congratulate ourselves that, owing to your labors as Fellows and to your distinguished leadership, we have realized all that could have been reasonably expected at the organization in eighteen hundred and forty-seven.

To one reading the history of that organization, it seems as if the Academy had been established when the heart of the profession was greatly stirred, when, as it appeared to many, it was in imminent peril from the onslaughts of the various forms of quackery. Of these, homœopathy receives the most frequent mention in its history. The inaugural addresses of the first few years contain many allusions that show that this was established as a place where the sheep were to be carefully separated from the goats, and heresy from the sound medical faith. This great subject was taken very seriously by our fathers, just as heresy in religious matters is taken in many other quarters to-day.

When the Academy was founded, the revolution of Continental Europe of 1848 was possibly in the air even of our city of New York, and as we had no immense political troubles, at least none involving our existence and form of government—the war with Mexico not having profoundly stirred us—our profession seems to have scented a great danger from afar and to have provided a rallying place against an iconoclastic enemy. That same Continental political storm brought to the Academy, by the way, one of its most illustrious members, the late Dr. Krackowizer, and, if I mistake not, several other men who have become eminent on these shores—one of them now an ex-President of the Academy. Be this as it may, in regard to the impregnation of the air with revolution, in reading our early history, one may almost hear the sound of the noisy preparations for building a great fort-

ress, or imagine the hurried and whispered orders of the formation of a line of battle in the face of the enemy.

The inaugural address of the first President, Dr. John Stearns, whose portrait is upon our walls, and whose residence at that time was at 84 White street, contains many eloquent passages in classical English, inveighing against impostures and innovations in medicine, and the doctrines taught by Hahnemann. Dr. Stearns took somewhat of a pessimistic view of the medical as well as of the religious situation in 1847. He asks, "Do not the state of the public mind and the signs of the times, in both the religious and medical world, indicate an approaching period when it will be inscribed over the portals of the Halls of Science '*Ilium fuit*'?" His sincerity and honesty of purpose, no one who looks upon his face, as it beams upon us, or who reads his fervent peroration, can doubt. "Could I be sure," he says, "of the uninterrupted and enduring prosperity of the Academy in disseminating health, happiness, and the sustaining principles of life, I should die in peace, with effusions of gratitude and praise to Almighty God for His permanent blessings upon our labors." It is in no spirit of criticism of this eminent and honest man that I have made these quotations. From his point of view, his anxieties and distress were well timed, and if he "builded better than he knew," in assisting in the establishment of this Academy, he was not unlike many others who have failed, in scanning the horizon, to hail the coming dawn which was to illumine much that was to them in utter darkness. In our time we are no doubt making errors in some of our methods, but we have outlived the dread lest impostors, innovators, and theorists shall overwhelm the science of medicine. In that direction, at least, we have made progress, although for a short time, at a comparatively recent period in the history of the Academy, it seemed as if heresy-hunting were to have a revival in the stormy scenes that have finally ended in the peaceable years of existence of our Academy, as a well-bred family

without a book of etiquette, in the omission from our by-laws of any reference to a formulated code of ethics. Does any one of my auditors believe that we have been less ethical from this omission?

In looking over this list of the Presidents of the Academy I found that it has been my privilege to have personally known every one of them, except the first, from whom I have just quoted: John W. Francis, Valentine Mott, the two Woods, Stevens, Cock, Joseph M. Smith, Parker, Batchelder, Watson, Anderson, Post, Bulkley, Austin Flint, Peaslee, Barker, which brings the roll down to that of the living, Purple, Jacobi, and Loomis. With the memory of each one of these men, something arises in my mind to stimulate and encourage me in the performance of the duties which they, without exception, so admirably performed for their day and generation. Since I was a medical student, I have been in the occasional habit of attending these sessions, and I have happened to be present when some of the scenes were thrilling. I heard the arraignment of Dr. Horace Green, for what seemed like a charge of malpractice, when, like a wounded lion at bay, he stood up in the aisle of the chapel of the University, where the Academy then held its sessions, and bade defiance, with a serene and sad face that I shall never forget, to those who were in all honesty, but, as I shall always think, improperly, accusing him. Among them was no less a person than the then venerable Valentine Mott, who, animated by a high sense of duty, was severely criticising the work of a man who has long since taken his place with those who have brought renown to American medicine. Then in 1861 I saw the great surgeon, whom we all love to speak of as Frank Hamilton, decorated with his brigade surgeon's straps, come in when there was a crowded meeting and stand along the wall of the same assembly-room, while he was on his way to the front, where he distinguished himself so gallantly, not only as a scientific medical officer, but as a brave man. In the hall of the Historical Society, I heard Marion Sims deliver

his famous, epoch-making paper on "Silver Sutures in Surgery." But it is not an hour to indulge long in reminiscences, interesting as they may be. The Academy of Medicine having now come, thanks to the labors of my predecessors and their executive associates, to enlarged facilities and great prosperity, you not unnaturally ask, what work shall it continue to undertake that it may fitly represent the profession of this city and this county? To answer this question fully I am not at all competent, but I hope to throw out some suggestions as to the work that the Fellows and Officers may unitedly endeavor to perform.

Although named an Academy, this association, as pointed out by Dr. Jacobi in his Inaugural Address, has no particular likeness to the French Academy of Medicine. It is not an association of a small number of selected wise men, who, in the leisure of advancing years, under favorable governmental patronage, devote themselves to scientific investigation and criticism of erudite medical theories, and who give authoritative decisions like a great high Court of Justice. In our country, medical institutions are usually unendowed, except by the private gifts of individuals, gifts to which they have usually been induced by the greater or less importunity of their family physician, and also, let it never be forgotten, by the legacies and donations such as those made to this Academy, in the Library of Purple and the money of Dubois and Farnham and Carpenter, of our own number. As befits our land, or, at least, as befits our present relations, as a profession, to the Government, it is a republican assemblage of chosen members of the profession, who have, at the least, passed the years of pupilage in schools, but who are hard at work earning their bread in the sweat of their face, by struggling to lengthen life and mitigate disease. For the most part, if not entirely, the papers and the addresses that are prepared for this Academy are written in the hours stolen from the imperative duties of an urgent and exacting professional life. Without an exception, so far as I know,

the executive officers of the Academy have been from this working class. Whatever more the future may demand in this great city, which is not only the commercial but the educational centre of this country, we now need for the cultivation of the science of medicine just such a broad and catholic organization as this, chosen from the working ranks of the profession, since an aspirant for membership must have been engaged "in active practice for the three years last preceding his application." It is a little more exclusive in its selection of members, but only a little more, than the County Society, and a little more ambitious in its aspirations. The New York Academy of Medicine thus fitly supplements the great legally constituted Society, embracing such a large proportion of the profession, which also meets in these halls. If the need for a more select, and, if you please, higher type of organization ever arises, no doubt it will be supplied; but at present, the Academy, I think you will agree with me in saying, comprehends about all that is necessary, in addition to the other societies and the great medical educational institutions of New York, for the advancement of medical science among us.

One of the most important things to which it seems to me this Academy should devote itself, in the present and in the future, is to secure for the medical profession a more authoritative position in all bodies of influence, political or social, in sanitary and hygienic matters. As I read history, the medical profession has lost somewhat in its authority over the educated public and the authorities of the State, since the mortal schism of the few years preceding 1847. A wiser treatment of that wild protest against the proud orthodoxy of medical dogma would have, perhaps, retained for us that which for so many years we have lost. It might have prevented us from being divided into what, however we may regard the division, the public at large have, until very lately, persisted in considering two sects. But this public, through the law-making powers, punished us for our quarrels by

stripping us of the exclusive right that we had so long enjoyed of determining who should practise medicine within our borders, and how they should be qualified. The recent law which happily, after years of contention, came upon the statute-books, and which has forced every person who wishes to practise medicine in this commonwealth to pass one examination, and thus all to have a common qualification, will in time, if allowed to remain unimpaired, and if supplemented by our own wise labors, restore us to our former position as the arbiter of the hygienic and medical interests of the State.

During these years that physicians have been contending with each other as to the credibility of dogmas and methods of practice, the profession has been gradually removed—with the notable exception of the army and navy of our country, where in all medical matters we still remain supreme—from the control of hospitals, Boards of Health, and from municipal commissions which look after great corrective and sanitary institutions. So far as I am informed, there are but two hospitals in this city, both comparatively small ones, where the profession has its due share of influence in the executive management. As we all know, it is utterly impossible to draw a line that shall separate that management from the proper care of the sick in these institutions. Of the Commissioners of Charities and Correction, three in number, one only is a physician. In the Board of Health there are two physicians, one being the Health Officer of the Port, always a political appointment; the other, thus far, always a representative member of this Academy, for whom we have never had occasion to blush. By one of the most extraordinary laws that it has ever been my fortune to have my attention called to in a civilized country, a physician is prohibited from being the president of this Board.[1] I have not heard, as yet, of a law which says that a man having a military education at West Point shall not com-

[1] Annual address by Dr. Andrew H. Smith, 1887.

mand the army, or that the admiral of the fleet shall have never been to sea; but, if this kind of legislation is persisted in, we may all live to see these things also made a part of the laws of the land. But this law does not seem peculiar to our fellow-citizens. But then they are not surprised that bankers and railway presidents and lawyers often have the chief place in managing hospitals. It is only a few weeks ago that a highly honored citizen, formerly mayor of this city, eulogized this provision of the law as to the Board of Health, in a public address. From this we may learn something of the tenor of public opinion, and of the best public opinion, not that of ward politicians, but of statesmen, with regard to the relations of the public to the profession that this Academy assumes to represent.

Take another instance: The office of coroner is one that, by any fair consideration of the proprieties, belongs to us. It is only by the chances of political nominations that coroners ever have a medical education. There is nothing in public opinion or in the law that demands that they should be physicians; but the deputies, who do the work, except that of holding the peculiar courts that coroners sometimes hold, are graciously but of necessity taken from the medical profession. The important duties of these deputies are, I believe, usually very well performed, but considering the technical education demanded, by any fair comparison to what is paid for professional labor to our friends of the legal profession, they are meagrely rewarded. This condition of things has come upon us while we have been discussing matters which would right themselves. The fault that it has come belongs, not to our fellow-citizens, but to ourselves. Proper respect is paid the medical profession in the household at all times. When danger of death is near, and in times of peril in the community, when an epidemic stares the city in the face, men who are glib—as glib as Molière—with gibes at the medical profession, are absolutely cringing in their desire to know what they shall do

to be saved from death. Almost white with terror, the lay members of a Board of Health then leave everything —forgetting for a while their politics—leave everything to the doctor. They know that in averting and arresting disease they are as helpless as would be a landsman in charge of the engines and the sailing of an ocean steamer on the North Atlantic. This condition of things in an hour of peril ought to exist in all matters pertaining to our profession in the time when, although the peril may not be seen, it is none the less imminent. I grant you that the chief work of this Academy is, naturally and properly, the scientific cultivation of the healing art, the promulgation of the discoveries made by educated experience; but having set these things forth, and being constantly in the habit of doing so, we must see to it that their being put into effect is not neglected. It is idle for us, immense in number and powerful in resources as a profession, as we are in this city, to be mere members of societies in which we promulgate doctrines of great importance to the public weal, if we have no power or make no attempt to put our views into action.

But the ignorant or unscrupulous enemies of sanitary science and hygiene are not idle. Every year in Albany bills are being introduced that, if passed, would sap the foundation of the structure of sound medical learning that has been, with so much pain and so much labor, in the face of so many difficulties, at last firmly laid by the medical profession of this State. Within the last few days a bill has been introduced—and I am sure you will share my chagrin when I tell you that this has been done by a member of our profession, who happens to be a member of the Legislature—which would, if passed, completely nullify the good effects of the statute now requiring a preliminary examination before a person shall begin the study of medicine. This bill clearly proposes to allow the student to take any part of his three years of actual study in the medical college to learn to read and write and spell.

Only a few weeks ago the Regents of the University another public body having to do with our vital professional interests, in which we are scarcely represented were assailed by a society, asking them to grant a charter to a medical college for medical missionaries. Such a college as was asked for would make a laughing-stock of all our other laws; it would discourage our undergraduate medical colleges that are now seeking to reach the highest plane of medical instruction, and would do more harm to medical education than this Academy could accomplish in a decade. A committee should, I think, be organized, which, with very little expenditure of time and money, could see what was being done or what was being proposed in Albany, and, if improper, bring it to the light, where it would be soon overwhelmed with the confusion entailed by our opposition. The Board of Health is the child of this Academy. Its first president was an honored and distinguished surgeon, and one of our presidents—Willard Parker—but in the present political composition and methods of that board the child could hardly be recognized by its own father.

We have recently elected our quota of delegates to our State Medical Society. That which has been accomplished for this State in medical legislation—and it is an important and essential work—has been done by that society single-handed and alone, while in the strictly technical work of our profession it will compare very favorably with any society in the land. The delegates from this Academy have hitherto borne their full part in that work. I venture to express the hope that the new delegation may accept their office with a due sense of its responsibility, and not fail to see that we perform our share of the duties of that illustrious society. Through these delegates, and through a committee, if the Academy chooses to appoint one, we may co-operate with the Medical Society of the County to maintain those of our laws that fitly represent our views, secure the repeal of the odious law in regard to the presidency of the Board of

Health, and perhaps, what is just now of the highest importance, influence legislation in the direction of perfecting our quarantine regulations, so that they may come under the direction of the general government, as your distinguished Special Committee has already advised the Chamber of Commerce, and from which committee, I believe, we await a full report to us. This opinion of your committee has also been publicly indorsed by two eminent Fellows of the Academy, Dr. Sayre and Dr. Thomas. There is no doubt that this report to the Chamber of Commerce has been received with great consideration, and we have good reason to hope that wise legislation, which shall do much to remove our quarantine from the arena of partisan politics and to save our country from epidemics, will be the result of this timely work.

Just such a situation with regard to the anticipated visitation of epidemic cholera once confronted this city, at the time of the delivery of the inaugural address of the President. This was in 1849, when Valentine Mott had just been elected for the first time; he was again President in 1857. Dr. Mott says: "Unquestionably, the prophylactic, or preventive treatment, is that upon which we should rely with most confidence. . . . As vices, crimes, and evils everywhere, when they reach a certain point, seem providentially to generate of themselves or suggest their own antidote, so we have here, in the spread of Asiatic cholera, a key or clue furnished which may be converted into a remedial measure of incalculable importance." Dr. Mott then goes on to argue against the crowding of human beings in filthy dwellings, but what I wish especially to quote to you at this time is this: "Long before the warm season comes upon us, therefore, gentlemen, it should be our bounden and constant duty to impress upon our fellow-citizens the absolute necessity of their thorough cleansing, ventilating, and disinfecting processes." He then goes on to speak of depopulation and temporary sequestration of the inhabitants of this city. The warning that Dr. Mott sounded when New

York had a half-million of inhabitants we may well, in the face of what is happening in Hamburg now, and what happened in our harbor and city last autumn, repeat here, so that it may be heard by two millions of people vitally interested, lest an epidemic of cholera reach us in the summer of 1893.

The other means by which this Academy may advance the interests of science are well known to you and practised by you. Here the papers are to be read, the demonstrations made, the discussions held, which testify to our interest and progress in the healing art. At the beginning of my service for you, may I, even if in repetition of what has been better said by some of my predecessors, be allowed a few words as to what the Academy seems to desire in the matter of technical contributions.

This is no place for long and dreary quotations from Germans, Frenchmen, or even Englishmen, with no additions thereto of original ideas on the part of the writer. Leave us, I pray you, in the calm and quiet of the late evening hours, in our libraries, to read by ourselves such *résumés* of what is being done on the other side of the Atlantic. They are published in the medical journals and are very accessible. A man or a woman who claims such a rostrum as this ought to have something new to say, and he ought not to consume too many minutes in saying it. He may assume that his auditors are tolerably familiar with what has been done by others on the subject of which he writes. What we are chiefly anxious to know is, wherein has the man or woman before us added to the knowledge of the world? How does he advise us to increase our usefulness? Besides the facilities afforded by the medical journals, the well-attended Sections of the Academy will allow of much more extended speech and quotation and discussion of foreign opinions than is proper here. This reading desk should be sacred to the promotion of new ideas. The days when medical literature in New York consisted of a few notes upon an English text-book and a translation of a French and German

treatise are passed, not to return, unless cerebral softening should simultaneously attack all the physicians who are Fellows of the Academy.

It was the aim of the architect of this building to so build it, for thus he was instructed, that there should be rooms in it devoted to social intercourse among the Fellows of the Academy. The smoking-room and dining-room are especially designed for this purpose. Perhaps we have not been long enough here to fully realize this idea of a medical club associated with a scientific body; but those who advised the architect, I am sure, remain very anxious to see the social amenities fully cultivated among us. The power of such an association—good acquaintance, the informal exchange of ideas belonging to it—ought not to be lightly regarded. The library, every year growing into more usefulness, is free to all members of the profession, or to any who desires to make any proper use of it: but the club feature ought, in my opinion, to be fully established and to be reserved for those who, by their continued payment of the dues, deserve extra privileges. Let us assemble ourselves together not only as scientific physicians, but also in good fellowship as brother-men.

This Academy was founded for the purpose of the advancement of medical science, and as we have determined by our broad constitution, "by such means as shall appear to its managers to be expedient and proper." There are some who take a semi-monastic view of the life of members of the medical profession. They would have us entirely absorbed in the treatment of disease, as it comes before us in the care of the patients of which we have the responsibility. Their discussions would be merely those interesting to their fellows in little circles, met to consider the local progress of scarlet fever, measles, diphtheria, typhoid fever, and so forth. This narrow view has never been held by the Fellows of this learned body. The advancement of medical science comprehends, as we well know, everything that can possibly be done for the hygienic

welfare of the community. In all this, the Academy is bound to have a serious interest, and to promote it to take active steps, as occasion may require. There are those who seem to have a horror of the least publicity on the part of the work of the profession—that publicity which naturally and properly comes to the other learned professions. While I am in complete sympathy with those who abhor the publication of anything that may have for its object an advertisement of the skill or wisdom of the individual, on the other hand I consider it of the greatest importance that we should make our work, as the sanitary guardians of the commonwealth, as widely known as may be demanded by the necessities of our time. If it be not well known, how is it possible to cause it to exert any influence? It is to be always assumed that the interests of the public are the same as those of the profession. The timid souls who distrust all progress that is made through publicity, contest, and struggle, may be divided into two classes, those so fond of their own ease that they can bear nothing which disturbs it, and those enjoying that to which they have no right, at the expense of the public which they ought to serve, and who wish to be let alone in their ill-gotten position. Personal contention, backbiting, malicious aspersions, are matters which are certainly far from the mind of any man who is a true member of a noble profession, but contention for a good cause, for a needed reform, if in a proper spirit, although vigorous, if the contention be against the fault and not against those who commit it, should never be avoided, when it stands in the way of progress in the right. First "pure, then peaceable." As to expediency, of which so much is said by those who would delay reforms and retard progress, we may say with Archbishop Whately, "Nothing but the right can ever be expedient, since that can never be true expediency which would sacrifice a greater good to a less." This is a vast field opening out to us as Fellows of this Academy. It never can be properly occupied unless by the personal endeavor and the personal loyalty of each one

of us who is here associated. That personal endeavor must be, as far as it lies in our power, to advance the great interest which we represent, and through which we shall be, in some measure, benefactors of our time. This will enable us to plant this profession firmly and securely in the affections and the respect of the community. Acting in this spirit we shall, successfully, under God, resist any attacks upon our integrity:

> "For self-dependent power can time defy,
> As rocks resist the billows and the sky."

Stated Meeting, February 2d, 1893.

MEMORIAL ADDRESS ON THE LATE JAMES R. LEAMING, M.D.

By J. LEONARD CORNING, M.D.

Mr. President and Fellows of the Academy:

It is seldom that the profession is called upon to endure so great a loss as that which it has so recently sustained in the death of Dr. James R. Leaming, of New York. For nearly half a century Dr. Leaming had practised medicine with success. What a panegyric is contained in that brief statement! Among all the liberal professions there is, I am convinced, not another which, while exacting the most definite knowledge of special subjects, makes such stringent demands on the whole man.

To be thorough in one's professional acquisitions, and to prove it in consultation at the bedside; to advance the interests of the profession both in its scientific and corporate capacities; and to observe the laws imposed by the most delicate sense of professional propriety and good taste, are things which are in accord with the best traditions. Such attributes undoubtedly carry with them the prerogative of fame, which, though acknowledged, or largely acknowledged, by the scientific minority, is still fame. There is, however, a form of distinction found likewise among physicians, which, while including much of the above, has also added to itself a wide sympathy with human affairs—an extra-professional sympathy, if one may so express it—bringing the possessor of the talisman as a votive offering the respect and regard of all conditions of men, quite irrespective of the accidents of occupation or social station. It is this latter phase of

distinction which belongs in a pre-eminent degree to Dr. Leaming.

In his affectionate intercourse with his children, in his cordial regard for the interests of the profession, in his solicitude for the welfare of his clients, in his social relations, and in the complete discharge of all public obligations he displayed a versatility of thought and feeling that was indeed remarkable. In this welding together of heart and mind we find the explanation of his love for art on the one hand, and his equally enthusiastic devotion to philosophy on the other.

Such a character demands more than a passing scrutiny; and if, with a view to greater chronological accuracy, I now proceed somewhat more in detail with the recital, it is because I am confident that by so doing I shall gain the cordial and sympathetic approval of the distinguished audience here assembled.

The name of Leaming, or Lemming, is of very ancient English date, and records concerning it are found in county annals, gazetteers, and old manuscripts. Probably the first instance of the name is found in an old Welsh poem reciting the deeds of prowess of an Anglican prince in a battle which occurred A.D. 547.

From the twelfth till the last century a number of families bearing the name of Leaming were residents of York, Essex, and Lancashire. What is of more immediate interest is the fact that Christopher Leaming, from whom is descended the subject of this biography, landed at Boston about the middle of the seventeenth century. He remained, however, only a short time in Massachusetts, but removed to Long Island, where he married.

Dr. James Rosebrugh Leaming, one of the direct descendants of Christopher, was born at Groveland, Livingston County, February 25th, 1820. His early education was obtained in the district schools of the neighborhood, and later he was sent to Temple Hill Academy at Geneseo.

Having early evinced some taste for scientific pursuits,

his father decided to allow him to study medicine. Accordingly he served, as was then the custom, an apprenticeship in the office of two physicians—Drs. Edward and Walter E. Lauderdale, of Geneseo. This was in 1845. In 1847 he matriculated at the University of New York, graduating from that institution in 1849. Immediately thereafter he established himself in New York, his first office being located in Waverley Place. Soon after this he became associated in hospital work with Dr. George P. Cammann, who in his day was a recognized authority on chest affections. This association with Cammann was the means of directing Dr. Leaming's attention to affections of the heart and lungs, a bent which he followed with untiring diligence for many years, and which was rife in him almost till the hour of his death.

Dr. Leaming has held during his long career many important positions in hospitals and other medical institutions. He has also expounded, both orally and by the pen, that branch of medicine to which so much of his energy was given. The servile tendencies of certain phases of pedagogy were, however, foreign to his whole mental make-up. In his writings, in his lectures, in his conversation he seemed constantly bent on suggesting new channels through which thought might flow to more perfect sequences. And how beautifully flavored with quaint humor and kindly facetiousness was his discourse on matters commonly regarded as prosaic! Only those who have heard him on such occasions can adequately appreciate the deftness with which he was able to ward off the soporific influence of an abstruse argument by a timely anecdote. Even his more elaborate writings evince in a striking manner this ability to clarify nebulous propositions by apt similes and timely illustrations.

Among his more important papers may be mentioned the following: "The Use of Muriate of Ammonia in Sun-stroke," *New York Journal of Medicine;* "Thuja Occidentalis in Malignant Disease," *New York Journal of Medicine;* "Therapeutics of Chloride of Ammonium,"

New York State Medical and Surgical Transactions; "Cardiac Murmurs," *New York Medical Journal*, 1868; "Discussion on Pneumonia," Bulletin of New York Academy of Medicine, 1865; "Pleuritis," Bulletin of New York Academy of Medicine, 1870; "Respiratory Murmurs," *New York Medical Journal*, 1872; "Plastic Exudation within the Pleura," Dr. Brown-Séquard's Archives, 1873; "Hæmoptysis," *Medical Record*, 1874; "Significance of Disturbed Action and Functional Murmurs of the Heart," Transactions of the New York Academy of Medicine, and D. Appleton & Co., 1875; "Physical Signs of Interpleural Pathology," read before New York Journal Association, 1877; "A New Classification of Phthisis Pulmonalis," *Archives of Medicine*, June, 1879; "Diagnostic Areas over the Human Chest," *New York Medical Journal*, April, 1887; "Contributions to the Study of the Heart and Lungs," E. B. Treat & Co., 1887; "Acoustics applied to the Human Chest in Physical Diagnosis," *New York Medical Journal*, January 26th, 1889; "Endemic Pleuro-Pneumonia," *Medical Gazette*, 1880; "Is Consumption Communicable?" *New York Medical Journal*, 1883; "Interpleural Pathological Products, their Causes, Significance, and Specific Relationship to Pulmonary Phthisis," *Medical Record*, 1888; "The Philosophy of Climatic Treatment of Chest Diseases," Journal American Climatic Association, 1887.

Besides these strictly scientific papers, he also delivered a number of public addresses, among which the "Memoir on Dr. George P. Cammann," read before this Academy, and an "Annual Address before the Alumni Association of the Medical Department of the University of the City of New York," are specially noteworthy.

Dr. Leaming was a member of many learned societies. He was one of the older members of the New York Academy of Medicine, of which he was also Vice-President. He was also for many years an active member of the Alumni Association of the Medical Department of the University of the City of New York, as well as President

of that organization. At the time of his death he was also a member of the Pathological Society, of the County Medical Society, of the Physicians' Mutual Aid Society, and Consulting Physician to St. Luke's Hospital.

Besides the above papers pertaining to the special field in which he labored, Dr. Leaming lectured in a number of medical institutions, and notably in the New York Polyclinic, of which he was President and Emeritus Professor of Medicine.

Leaming will always be regarded as one of the cleverest diagnosticians of heart and lungs; and the profession will also remember him as the advocate of large doses of calomel in the treatment of pneumonia, and smaller doses in the treatment of pleurisy. So confident was he of his own ability to ferret out the more occult features of cases that, shortly before his death, he expressed, as Dr. Frank W. Jackson informs me, great regret that he was unable to apply his ear to his own chest.

Leaming was a man who had the courage of his convictions. This was impressively exhibited during an attack of pneumonia from which he suffered several years ago, when on two successive days he took a drachm of calomel at a dose.

Beyond all doubt Dr. Leaming's greatest services to the profession were his teachings in regard to pleural pathology and the interpleural origin of râles. When Dr. Leaming began to teach the interpleural origin of râles, he was confronted by a storm of opposition which would have dismayed a man less sincere in his convictions. Thus a prominent diagnostician, since deceased, declared at the time that it was impossible that râles could be produced in the pleura. Persevering in his advocacy of this theory, however, he lived to see his propositions meet with widespread acceptancy in the profession.

While it is probable that few diagnosticians will be willing to accept his dictum that all râles have their origin in the pleura, it is, as previously said, generally admitted that they may, and often do, thus originate.

In his paper on "The Significance of the Crepitant Râle," Dr. Frank W. Jackson observes:

"He (Leaming) bases his argument for the pleuritic origin of râles in pneumonia, apart from theoretical considerations, upon the fact of the presence of fibrin on the pleura in these cases, and the very significant fact that râles were heard shortly before death, when, on autopsy, the lung was found solid from pneumonic consolidation. Here it is obvious that no air could enter the air vesicles to disturb them or to produce bubbles in a viscid fluid." His researches regarding pleuro-pneumonia in cattle also served to yield confirmatory evidence.

By common consent Dr. Leaming was credited with an ear which, in its acuteness, was almost without a rival.

In the above array of statistics we have a brief summary of the scientific work accomplished by Dr. Leaming in his long professional career; but how inadequate is this exposition as an index to that fulness of living, that breadth of sympathy in the great and little things of this life, which was so characteristic of him. If it be difficult to give even a partial idea of the intellectual attainments of such a man, how much less competent shall we be to give an inventory of that rich soul-life which constantly prompted him to deeds of kindness and heroic effort in behalf of his suffering fellow-men!

Among all his characteristics I think his love for the beautiful, his devotion to the fruits of the imagination, whether expressed in color or in marble, in landscape or in music, was most lovely. I remember finding him one day, as the drama of life was drawing to a close, sitting in a large easy-chair surrounded by the works of Bacon, Huxley, and Spencer, by the poetry of Chaucer, Shakespeare, and Byron, while from the walls of his study the faces of old cavaliers gazed upon the scene with looks suggestive of the tournament or the battlefield. I remember, too, we were discussing that beautiful poem of Byron's, from the "Hebrew Melodies," beginning:

When coldness wraps this suffering clay,
Ah, whither strays the immortal mind?
It cannot die, it cannot stray,
But leaves its darken'd dust behind.
Then, unembodied, doth it trace
By steps each planet's heavenly way?
Or fill at once the realms of space,
A thing of eyes, that all survey?

Eternal, boundless, undecay'd,
A thought unseen, but seeing all,
All, all in earth, or skies display'd,
Shall it survey, shall it recall:
Each fainter trace that memory holds
So darkly of departed years,
In one broad glance the soul beholds,
And all, that was, at once appears.

Before Creation peopled earth,
Its eye shall roll through chaos back;
And where the furthest heaven had birth,
The spirit trace its rising track.
And where the future mars or makes,
Its glance dilate o'er all to be,
While sun is quench'd or system breaks,
Fix'd in its own eternity.

Above or Love, Hope, Hate, or Fear,
It lives all passionless and pure;
An age shall fleet like earthly year;
Its years as moments shall endure.
Away, away, without a wing,
O'er all, through all, its thought shall fly;
A nameless and eternal thing,
Forgetting what it was to die.

As he finished reading the last line the door-bell rang, and presently there was ushered into his presence a young man, evidently in the last stage of phthisis. He wore that wistful, forlorn expression peculiar to persons in his condition, and as Dr. Leaming finished the examination the patient looked at him with an expression in which apprehension and hope formed a sad incongruity. His condition was so clearly hopeless that I inwardly trembled at the thought of what effect a sudden announcement of his true condition might have upon him. Instead of giving

him a direct answer, Dr. Leaming rose, and without the slightest trace of embarrassment, and a benevolent look, conducted him about his consulting-room, and thence into his drawing-room, exhibiting with an air of enthusiasm, and in the most charming anecdotal manner, the pictures which he had been so long in collecting and which were so much to him. The effect upon the patient was magical: his face at once assumed a more cheerful aspect, and when the consultation was finally at an end and Dr. Leaming re-conducted him to his study, nothing but the hopeful expression sometimes seen in persons suffering from this fatal affection was visible. This is but one of the many instances in which this soulful man was able by his masterful sympathy and tact to remove the sting of a relentless fate.

But, considerate though he was of the interests of those who were endowed with the riches of this world—and there were many such who constantly sought his advice and skill—it was above all when confronted with the misfortunes of some poor client, some waif of fortune cast at his feet by the billows of adverse fate, that his great-heartedness was most completely revealed.

Sometimes it was a superannuated clergyman, who, while favoring him with a dissertation on the cosmogony of Moses, would interpolate a hint that the cause of sound theology would be much advanced by a modest donation. Again it was a lawyer without clients, or an inventor with a patent which no one could be induced to accept as a gift, much less pay for; or, perhaps, saddest of all, it was a doctor, worn out by the arduous duties of his profession and left in the evening of life—for no crime or personal omission—without health, houseless, hopeless, stranded. Ah, what words of ours shall show him to you at such a time as that! How can I paint him, as he sat in the well-worn chair, his brow lined with the records worn by thought, his locks silvered more by the care he took for others than by years? With what looks of tenderness and tremulousness of voice did he work his spell

on the unfortunate, and rested not in word and deed till the demon of want had fled in very shame from the field!

Noble as were his feelings toward the poor and afflicted, the sentiments which he entertained with regard to his strictly professional obligations were, if possible, even more exalted. So sensitive was he in regard to matters which in the smallest degree involved the rights of others that he would frequently sacrifice his own interests—unnecessarily, as it seemed to some—rather than incur the possibility, however remote, of misunderstanding. This is a fact so well known as to have obtained the grateful recognition of his *confrères* throughout the length and breadth of the profession.

Leaming's honor was indeed like that ascribed to Cæsar's wife—beyond the possibility of suspicion.

His conception of his duties to the profession did not stop, however, at a mere abstention from interference with the rights of others; on the contrary, during his long career as a successful practitioner he was constantly on the alert to aid in every way those who seemed worthy of his support. This was especially shown in his relations with the younger members of the profession, in whose several destinies he seemed to take an interest as sincere as it was unusual.

Particularly astonishing was the accurate knowledge which he possessed of the genius or acquirements of so many of the younger men.

I remember asking him one day how he was able to construct such an apparently faithful analysis of the mental resources of so many persons.

"You see," he replied, "I have been debarred of late years by indisposition of a physical sort from taking as active a part as I could wish in the work of the medical societies. But, in spite of this, I have been able to keep myself in pretty close *rapport* with what is being done by the younger men through the medium of the journals." Then, rising, he showed me a chest of drawers filled to repletion with journals and scrap books, whose monstrous

store of clippings threatened momentarily to burst the bindings.

"These," he continued, "are my 'Doomsday Books,' and though they have cost some time and labor to arrange, I feel quite repaid, since at a moment's notice I am able to refer to most of the good things which have been done by the younger men during the last ten or fifteen years.

"In this way I am able to distribute such patronage as I have where it will do most good to the individual and hence to the profession. For," he added, "I conceive it to be the duty of all physicians who are well established in practice to aid in every way possible those of the younger men who by their talents and application are really advancing the art."

At the time he gave utterance to this high-minded and disinterested sentiment he was stricken by the infirmities of advancing years and by serious disease.

Truly, a finer example of conscientious devotion to the higher ideals of the profession, in the face of what to smaller minds would have been irresistible temptation to pessimism or *laissez faire*, has rarely been witnessed.

We have already dwelt upon the truly altruistic spirit in which Dr. Leaming met every reasonable, or even unreasonable, demand from without. But if his learning, professional fidelity, charity, magnanimity, and disposition to give an expansive meaning—a poetic interpretation—to even minor events of life have awakened unmingled admiration and respect throughout the profession, what pæans would have been sung to him could all have been witnesses of his rich home life, all aglow with that broad hospitality, that mutual deference which springs from a heart that intuitively recognizes the fraternal relations of the whole human race! Kindness, all oblivious of self, was with him an instinct. Like some ancient Christian father, whose deeds of devotion were performed amid the darkness of the Catacombs, with none to witness his heroism save the eyes of the Omniscient,

Leaming was ever ready to imperil his own interests in behalf of his fellow-men, without a thought of encomium, from which, indeed, he naturally shrank.

What wonder that the smiles of such a man should have drawn to his table all that makes for the best in our metropolitan society! Men high in scientific and literary achievements, in politics, in art, found in his house that warmth which makes the heart glow.

Thus, only a little while before his death there was assembled one evening in his drawing-room a distinguished company. Among his guests was the President-elect of the United States, whom the hospitable host was able to regale with an exhilarating decoction drawn from a punch-bowl which originally belonged to Thomas Jefferson.

Is it possible to imagine a more delicate compliment from a host to his guest than the tribute which Dr. Leaming accorded Mr. Cleveland that evening?

The same gentle relations that he sustained to his patients he also maintained with his *confrères* in the profession. In this we perceive the source of his power and popularity as a consultant. It would, indeed, be difficult to say too much in praise of the tact which he displayed when called upon to perform this difficult function. The modest dignity of his address, the sapient glance, the benevolent smile, the reassuring words, and the unostentatious commendation of the efforts of the attending physician—what words shall do justice to our appreciation of it all? Truly an instinctive gentleness, a spontaneous interest in and regard for others, made up the fundamental tone in his character. To infer from this, however, that he was devoid of the sterner virtue of courage, would be a grave mistake.

Dr. Charles W. Packard, who did all that active and skilful solicitude could accomplish for his comfort during his last illness, informs me that never in his long experience had he been a witness to greater fortitude and cheerful acquiescence in the decrees of the inevitable.

It was also my privilege to have seen him on several occasions in consultation with Dr. Packard, on account of the delirium and persistent insomnia which developed from time to time toward the last.

On these occasions I was also able to learn from his wife and daughter, who cared for him with rare devotion throughout his long illness, of that heroic spirit with which he was able to endure the severe suffering which destiny had in store for him. At last came the inevitable crisis, and on December 5th, 1892, at 7 o'clock in the morning, his career among mortals was ended.

In a narrative culled from the rich archives of oriental allegory it is related that, as a certain king lay dying, he complained of an oppression which the magicians of the court were unable to exorcise even with their most cunning incantations. When all were in despair there suddenly appeared a poor minstrel, upon whom the king one day, finding him wounded about the chest by robbers, had lavished costly ointments. Reminding the stricken prince of his former bounty, the minstrel, with tearful eyes, played upon his harp an air of surpassing loveliness, when immediately the dying man made known that all distress had left him, and he passed away as though in sleep.

Ah, could those suffering breasts to whom Leaming brought relief have sung his praises at the supreme moment, on what mighty waves of harmony would his spirit have been carried onward into the ocean of eternity!

I know that in discourses of this kind it has been the practice, in concluding the recital, to make an attempt at elaborate psychological analysis; to place in the scales, as it were, each mental trait, and then, by a simple process of addition, to find a more or less consistent explanation—usually less—for the thoughts and emotions of the subject, as well as the actions which were their direct issue. For my part, even though I had an inclination for such necromancy, I should certainly withhold my hand

in the case of the man who constitutes the subject of this biography.

Dr. Leaming was indeed one of the loveliest, and at the same time one of the most picturesque, personages to be found throughout the length and breadth of the profession.

A lover of precious stones, of which he had made a large collection, he little realized that his own character was the rarest gem of all.

So unique a personality should be dealt with in the spirit which makes for poetry rather than prose. In the brief recital just ended I have endeavored to show him to you as he was, not only just, but magnanimous; not only a medical philosopher, but a lover of the Muses; not only sought for by the rich, but adored by the lowly, upon whom he preferred to shower benefits without the presence of an applauding audience.

Just how much of this rare combination of strong and lovable characteristics was due to heredity, and how much to fortunate coincidences of environment, let each decide for himself.

To me he seemed like an exceptional and beautiful creation—as are all great and good men, to remind us of the immeasurable possibilities of human nature.

Such attributes of the spirit cannot die; for though the physical casket in which they were enshrined for a season may pass away, their essence shall remain forever in our hearts as a glowing memory.

ESTABLISHING A NEW METHOD OF ARTIFICIAL RESPIRATION IN ASPHYXIA NEONATORUM.

By J. HARVIE DEW, M.D.

THE best method of practising artificial respiration in asphyxia neonatorum has, within a comparatively recent period, been made a subject for discussion by Dr. Lusk's most excellent paper, read before this Academy in December, 1890, and published in *The American Journal of the Medical Sciences* for February, 1891; and more recently by Dr. W. E. Forest's article in the *Medical Record* of April 9th, 1892.

Prof. Lusk offered no new method, but discussed and made more prominent the method known as that of Schultze, and impressed the importance of the use of the catheter for the removal of mucus and foreign matter from the trachea and larger bronchi, and also its use in artificial respiration by direct inflation.

In the discussion which followed this paper I took occasion to state that for many years I had practised a method which I regarded as original, and which I believed to be of great practical value. I then briefly described my method.

Dr. Forest, in the *Medical Record*, finds all of the adopted methods in one way or another faulty, and presents one of his own (another modification of Sylvester) as "filling the bill."

Of my statements before the Academy he writes: "Another method that has been widely advocated and practised, and yet is utterly worthless, is Schröder's. At a recent meeting of the New York Academy of Medicine a physician present [referring to myself] claimed that this method was most efficient and put it forward as original

with himself. In both cases it will be seen he was mistaken."

Dr. Forest then describes "Schröder's well known method," as he terms it, not exactly as Schröder himself describes it, but rather approximately, as I described mine before the Academy in 1890.

In this paper I desire to show: 1st. That Schröder's method is no more like mine than mine is like that of Schultze. Also, that the principle and the object of all three of these methods—viz., those of Schröder, Schultze, and my own—are nearly or about the same, while the plan of procedure is entirely different. 2d. That my plan is original, having been adopted independently of any idea secured from text books or elsewhere, and that I practised it in 1871, or early in 1872, at least two years before Schröder's first publication of a method, which appeared in 1874, and before I had ever known of Schultze or of his method, which was published in 1871. 3d. That my method is the promptest, most powerful, and most efficient for immediate use which can be practised with perfect readiness and ease to the operator.

Schröder, in his work on obstetrics, 7th edition, 1882, seems to especially advocate Schultze's method, which should be performed by "grasping the infant in such a manner that the operator's thumbs shall rest on either side upon the anterior thoracic wall, while the index finger occupies the axilla and the remaining fingers are placed diagonally across the back. The child is then allowed to hang at arm's length between the knees of the obstetrician, its face being turned to the front. The child is next swung upward until the arms of the operator reach an almost horizontal position." These two movements induce inspiration. "The swinging motion is then arrested, flexion occurs in the child's lumbar spinal region, its head is directed downward, and its lower extremities fall slowly toward the obstetrician until the whole weight of its body rests upon his thumbs." This movement induces expiration.

Schröder next speaks of catheterization for the removal of mucus, etc., and in presenting what is termed his method advocates it in no terms of special commendation, but apparently in the most casual way. He simply says: "If no foreign particles have been breathed in, or if they have already been removed, then one can employ artificial respiration in still other ways, thus: While the babe is in a bath one can enlarge the thorax, in that one supports the back of the babe while its head, arms, and pelvis fall backward; a forceful expiration is then effected by bending up the baby over its belly, thereby compressing the thorax." This is literally all that Schröder has to say of this method for which he has been given credit.

Francis Henry Champneys, in his article reporting his experiments on the cadavera of still-born children, published in the "Medico-Chirurgical Transactions," dated 1881, condemns Schröder's method as worthless. He presents the method as follows: "Schröder suggests" [please note the fact that Champneys does not say that even Schröder himself ever advocated or practised this method, but merely that he suggests] "supporting the child by the back only, letting the arms and legs fall backward, which will produce opisthotonos; and then bending them in the contrary direction, producing emprosthotonos—the latter to produce expiration, the former inspiration."

The only explanation of Schröder's method I have been able to find in any English or American text book appeared in the first edition of Lusk's work on obstetrics, and he has not thought it of sufficient value to reproduce it in subsequent editions. Yet Dr. Forest says in his article that it "has been widely advocated and practised,' and at the same time quotes Champneys and declares it worthless.

I do not believe that any one could successfully practise this method as Schröder directs, or that it has ever been to any extent taught or "advocated."

The plan I have adopted is no more like Schröder's

than those of Pacini, Bain, Schücking, Schüller, and Forest are like Sylvester's.

I mention this fact because there is one most important difference to which I desire to call attention. It is, that all of these methods named have been suggested by, and are simply, to a large degree, modifications of, that of Sylvester, while my plan was suggested by no previously adopted method. At the time I began to practise it Schröder's method (which most nearly resembles mine) was not in existence, or certainly had not been published. Indeed, it was not published till 1874, two years or more afterward; and at this time Schultze's method was in its infancy and entirely unknown to me.

I cannot now remember exactly when I began to practise this method. The first instance I can well authenticate occurred in the practice of Dr. George T. Harrison, of this city, who is present with us to-night.

I was called by him to assist in a difficult case of labor which he was attending in the old building that stood at the corner of Fifth Avenue and Fifty-fourth street, on the ground now occupied by the handsome "Vanderbilt" residences of Twombley and Webb. This was either in the latter part of 1871 or the early part of 1872. The infant at birth was thoroughly asphyxiated. The work of resuscitation devolved on me. It took considerably over an hour to fully establish the respiratory process. My method was then practised to relieve myself of the fatigue which that of Sylvester imposed, so the two methods were alternated during the treatment. I have continued its use, almost exclusively, from that time till now.

Description of Method.—My directions for its practice are: To grasp the infant with the left hand, allowing the neck to rest between the thumb and forefinger, the head falling far over backward, straightening the mouth with the larynx and trachea, thereby serving to raise and hold open the epiglottis (as demonstrated by Benjamin Howard in his excellent article, "A New and Only Way of

Raising the Epiglottis," *British Medical Journal*, November, 1888). The upper portion of the back and scapulæ resting in the palm of the hand, the other three fingers to be inserted in the axilla of the baby's left arm, raising it upward and outward.

Then, with the right hand, if the baby is large and heavy, grasp the knees in such a way as to hold them with the right knee resting between the thumb and forefinger,

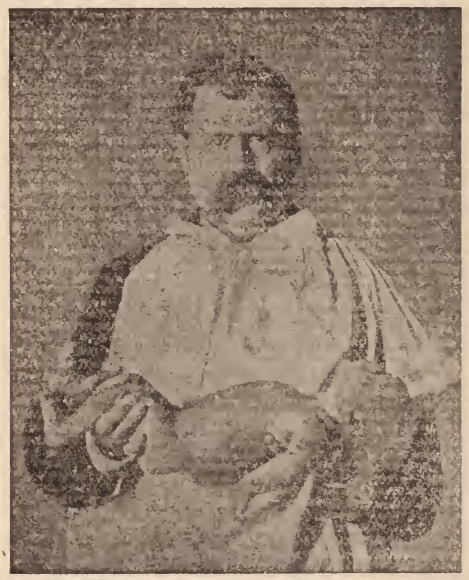

FIG. 1.

the left between the fore and middle fingers. This position will allow the back of the thighs to rest in the palm of the operator's hand. If the infant is small and light, it will be found more convenient and easier to hold it in the same way by the ankles instead of the knees, allowing the calves instead of the thighs to rest in the palm of the hand.

The next step is to depress the pelvis and lower extremities, so as to allow the abdominal organs to drag the diaphragm downward, and with the left hand to gently

bend the dorsal region of the spine backward. This enlarges the thoracic cavity and produces inspiration.

Then, to excite expiration, reverse the movement, bringing the head, shoulders, and chest forward, closing the ribs upon each other. And at the same moment bring forward the thighs, resting them upon the abdomen. This movement arches the lumbar region backward, and so bends the child upon itself as to crowd together the con-

Fig. 2.

tents of the thoracic and abdominal cavities, bringing about a most complete and forcible expiration.

While this movement is a powerful one, the operator can, by his manipulations, accomplish it without shock and render it as gentle as he pleases.

It is evident from Dr. Forest's article in the *Medical Record*, already alluded to, in which he condemns Schröder's method as worthless, that the plan I propose will be denounced by some one, and for the same reasons that Champneys assigned in discussing Schröder's method.

I therefore think it best to state that Champneys' experiments were made by securing one end of a tube in

the larynx of a still-born cadaver, while the other end was immersed in a fluid. He then endeavored, by exciting the inspiratory act, as directed in the different methods of artificial respiration, to determine by the ascent of the fluid how much air each one of them was capable of sucking in.

His experiments led him to the conclusion that the methods of Schultze and Sylvester will suck in the greatest amount; that the modifications of the Sylvester

FIG. 3.

method are the next most powerful, and that those of Howard, Marshall Hall, and Schröder excite no perceptible suction, hence are worthless.

These experiments were all made on the cadavera from twenty-four to thirty-six hours after birth, when rigor mortis had, or certainly had had time to, become as fully established as is usual in such cases. Under these circumstances it seems to me easy to understand that the action and forcible procedure incidental to the Schultze and Sylvester methods would exhibit a decided suction, while the much more passive movements of the Howard

and Marshall Hall methods, and the plan "suggested" by Schröder, would draw in no appreciable amount of air. I cannot see how any other results could have been expected.

Only think! Take an infant thirty-six hours dead, with the muscles, ligaments, cartilages, and even the bones to a certain extent stiffened or deprived of their elasticity, place a tube in its larynx as stated, and then, with a hand supporting its back, allow the head and shoulders to gravitate to one side, its pelvis and legs to the other. Now, how much air do you suppose is going to be sucked in? I should certainly answer, none, even if I had never heard of Champneys and his experiments.

Finding, by this exceedingly passive movement of producing opisthotonos in the cadaver, that no air was drawn in, Champneys at once proceeded to account for the failure by stating that this method (Schröder's) is based upon the *a priori* reasoning that the capacity of a flexible cylinder increases when it is curved, and concludes as follows: "But the principal reason lies in the fact that, in children who have never breathed, the position of the thorax is one of expiration and not of inspiration, the thoracic walls are completely collapsed, and there is no thoracic cavity or cylinder to deal with. There is nothing to procure descent of the diaphragm, and the thoracic cavity cannot be expanded in any direction by such means."

Some of the features of the plan I have adopted are more like to Schröder's than to any other thus far published. This fact, and the comments that have been made, have led me to direct your attention thus especially to what Schröder's so-called method is, and to what has led Champneys to condemn it. I do not think these conclusions will apply at all to my method, and I hope you will all agree with me when I present to you the following explanation:

At birth an asphyxiated infant is perfectly limp and flexible. Its muscles are like so many wet rags, and offer no resistance till stretched out to near or about the limit

of their elasticity. In the Sylvester method the ribs are not lifted till the pectoral muscles have been put well upon the stretch, for the accomplishment of which the arms must be forcibly pulled upward.

When this is done the chest cavity is increased laterally, and the diaphragm is flattened out, pressing the abdominal organs to some extent downward, thus serving, in a measure, to increase the cavity vertically. This produces the suction which every one recognizes, and which has made this method, up to date, the most universally known and adopted.

My method accomplishes exactly the same results in a different way. To understand how it is done, let us consider for a moment the anatomical structure of the chest walls. These walls are supported by, and have their fixed point in, the attachment of the ribs to the dorsal vertebræ. They are composed mainly of the ribs, their cartilages, the sternum, and the intercostal and pectoral muscles.

The muscles, as stated, offer no resistance and no assistance, except for traction.

The ribs constitute not only the most prominent structure in the formation of the chest walls, but their movements are essentially important in any effort artificially or naturally to draw air into the lungs. It is upon their peculiar arrangement, formation, and attachments that the active inspiratory movement of my method depends.

They are twelve in number on each side, and are separated from each other at well-defined distances. They vary in both length and shape, from the first to the twelfth. They can be made to very closely approximate, if not to overlap each other, and are capable of as wide a separation as the elasticity of the intercostal muscles will permit. They terminate at the sternum in flexible cartilages, which vary in length and render them very movable.

Posteriorly they have almost a fixed attachment. Their heads are closely bound by a strong ligamentous union to the bodies of the dorsal vertebræ, while their tubercles,

located nearly an inch from their heads, are bound with equal firmness to the lateral processes of the same vertebræ. Only a slight rotatory motion exists at this articulation, which, together with the peculiar shape of the ribs and the flexibility of their anterior attachments, enables the normal inspiratory act to be performed, the ribs at each effort being drawn upward and outward.

Now comes the important fact I wish to impress: it is, that in my method of artificial respiration, owing to the firm attachment of the ribs to the bodies and processes of the vertebræ, as soon as the dorsal region is curved backward and the relative position of the bodies and transverse processes are changed, the ribs and their intercostal muscles open out like the segments of a fan, and at the same time, owing to their peculiar shape, all of the bodies of the longer ribs are forced outward and the diaphragm is flattened. Thus, both the lateral and vertical diameters of the thoracic cavity are increased.

How much air is actually drawn in and how much reflex action is excited by the inspiratory effort of this or any other method in the first few moments, it is difficult, from a clinical standpoint, to determine; but after keeping up the operation for a few moments, in any favorable case, it will be easy to recognize unquestioned evidences of suction.

The infant whose photograph I herewith exhibit was born a little before, and died a short while after, 7 P.M. I was not present at its birth, but reached the bedside a few minutes after death. The photographs were taken at 11 A.M., sixteen hours after death. I then performed my method of artificial respiration, and was able to force air in and out of the lungs with each movement. The evidence was made positive by a very audible sound, excited by the escape of air at each expiratory effort.

It is very frequently, if not usually, the case, when resuscitating an infant, that a decided grunt is heard with the expiratory movement, after once the introduction of air has been established.

The expiratory movement in this method is one of its most perfect and advantageous features. Indeed, I believe that a complete expiration is, for at least two reasons, of equal if not of greater importance than that of inspiration: 1st, because if one cubic inch, or any given amount, of air is drawn in, it is most desirable that all of it shall be forced out in the movement that follows; 2d, because the expiratory effort in artificial respiration should not only serve for the expulsion of air, but should at the same time be a means of improving and hastening the general circulation.

If the thoracic cavity is thoroughly but gently compressed, the heart and large blood vessels will be unloaded in the direction of least resistance—this, of course, must be forward and in the right direction, as the cardiac, aortic, and pulmonary valves will open for its forward and close upon its backward flow. The accomplishment of this result with each expiration cannot be otherwise than most beneficial to the sluggish circulation of an asphyxiated infant.

The Schultze method fills this requisite, as pointed out by Dr. Lusk in his article upon this subject; but the action is too violent and cannot be regulated with gentleness. The Sylvester method and its modifications, the mouth-to-mouth insufflation, and inflation by catheterization are all deficient in this particular. They, each of them, depend for their expiratory movement on lateral pressure over the lower ribs, upon the epigastrium, or both together. This plan of expiration is objectionable because: 1. It does not expel all of the air from the lungs, if any has been drawn in. 2. It causes the centre and posterior portion of the flabby diaphragm to descend, thereby increasing the vertical diameter of the chest cavity. 3. It produces but slight, if any, pressure upon the heart and large blood vessels which occupy the mediastinum; certainly not sufficient pressure to be of any material benefit to the circulation.

In the expiratory movement of my method, when the

shoulders and chest are brought forward, and at the same moment the thighs are made to rest upon the abdomen, including the epigastric region, the pressure upon the contents of the thoracic cavity can be made as forcible as the operator thinks best. The ribs are crowded upon each other, closing up the intercostal spaces, and the organs of the abdomen are pushed upward upon the diaphragm so as to diminish the vertical diameter as much as it is possible to do. By these combined forces the

FIG. 4.

expulsion of air is complete, and the desired effect upon the heart and large blood vessels is most favorably secured.

Every obstetrician who finds that he has delivered an asphyxiated infant proceeds at once to excite the respiratory act by reflex stimulus. To do this he moves the infant from side to side, spanks it, sprinkles water upon it, and possibly dips it alternately into hot and cold water; but when the asphyxia is too profound to be thus relieved, he is forced to resort to some one of the many methods of artificial respiration.

Of the established methods, Sylvester's and the plan of mouth-to-mouth insufflation are probably by far the most universally adopted, next that of Schultze, then catheterization and insufflation, and finally the individual plans not commonly known. One or more of these methods must be resorted to by every practitioner, hence it is a matter of unquestioned importance to be able to select the best among them, not only for individual use, but for instruction in our schools of medicine.

As previously stated, the Sylvester and the mouth-to-mouth plan offer good inspiratory but very imperfect expiratory movements. The Schultze method, though very efficient, is often inconvenient for want of space, is too chilling to the infant, and in many instances is too violent in its movements. Catheterization and insufflation is not easy, and is, as a rule, unsafe in inexperienced hands. Of the individual and private methods I have nothing to say, except of the one under consideration.

In maternity hospitals, where the obstetrician is offered every facility, and in the homes of the wealthy, where there are so many conveniences, the difference between the methods of artificial respiration may not be a question of so much importance; but in that very much larger class of cases occurring in the homes of the middle and poorer people, where there are but few conveniences, he must always endeavor to select the most ready and favorable plan for immediate use.

In any prolonged case of asphyxia the operator will become greatly fatigued in constantly pursuing any one of the methods proposed, and will find great relief in practising first one plan and then another.

Advantages of the Method.—I claim for my method the following facts and advantages:

1. That it is most efficient in all cases where artificial respiration in asphyxia neonatorum is indicated.

2. That years of experience have served to prove to others, as well as myself, its unquestioned value.

3. That it can be practised with ease and readiness to the operator.

4. That its movements are easy and can be quickly resorted to, at any moment and anywhere.

5. That while its inspiratory movement will be found, by experience at the bedside, to be as efficient as that of other methods, the expiratory movement is far more complete and satisfactory than in any of them.

6. That nearly or about all of the air drawn in can be expelled.

7. That owing to the force, and at the same time to the absolute control, which the operator has over the expiratory movement, he is able to compress the contents of the thoracic cavity to just exactly that degree deemed by him wisest and best, thereby favoring and hastening the general circulation.

8. That the operator can sit or move from place to place about the room, greatly to his relief from fatigue, still continuing the respiratory movements.

9. That, if thought best, the movements can be kept up while the infant is immersed up to its chin in hot water.

10. That by elevating the buttocks and depressing the head and shoulders, the expulsion of mucus can be effected as in the Schultze method.

11. That for alternating with Sylvester's and other methods it possesses peculiar advantages, affording great relief to tiresome positions in protracted cases.

12. That it possesses all of the advantages of the Schultze method and none of its disadvantages.

13. That the method is prompt, reliable, easy to perform, and perfectly safe.

DISCUSSION.

DR. G. T. HARRISON.—I can fully corroborate the statements which we have just heard in this paper of Dr. Dew as to the inestimable value of this method which he has devised for the resuscitation of the asphyxiated new-born child. I am convinced of the value of the method, not only from seeing it demonstrated by Dr. Dew

himself, but also from clinical evidence in my own practice. In the treatment of the severer types of asphyxia neonatorum the indications, I scarcely need remind you, to be fulfilled are, first, to remove all foreign substances from the air passages; secondly, to supply the lungs with air; and thirdly, to awaken the dormant energy of the heart and circulation. All three of these indications are fulfilled by this method of Dr. Dew. Some authors insist upon it that before we resort to any method of artificial respiration it is imperatively demanded to resort to aspiration of the mucus and foreign substances in the air passages by catheterization. In those instances in which I resorted to Schultze's method and Dew's method, I have never found that necessary.

With regard to Schultze's method, I can say from abundant experience that it fulfils these three indications in the most beautiful and exact manner. It cannot be denied, however, that this disadvantage will always attach to it, and that is that, in the hands of inexpert operators and general practitioners with limited knowledge, there is danger in its employment of injury to the child. Cases are on record in which fractures of the clavicle and ribs have been inflicted; nay, even rupture of the liver has been reported in a number of cases. And in the edition of Schröder's work edited by Olshausen and Feif, the statement is made that in one instance which came under their observation the spleen was detached in the abdominal cavity, and the peritoneal cavity was found to contain a considerable amount of blood. Dew's method, I think, is free from any such dangers. As he says, there are no such dangers inherent in it. I see no objection, though, and such has been my practice in cases of extraordinary difficulty, to resorting to both methods—both Dew's method and Schultze's method. But as a general rule I think Dew's method will supersede the necessity of resorting to any other. In the system of obstetrics edited by Hirst, I see allusion to a method adopted by Dr. Goodell. I think Dr. Goodell has a paper on that subject. The account given there is rather meagre, but in some respects it seems to be something like Dr. Dew's method. As for the method of Schröder, which Dr. Dew has dwelt so much upon, I may say that in all modern text books written by German authors, with only one exception, that method is not spoken of at all. The method of Schultze is the one that is universally adopted in Germany.

Dr. Von Beverhout Thompson.—It gives me great pleasure to indorse Dr. Dew's method of artificial respiration, which has been so fully and clearly demonstrated this evening. Of all the methods described, recommended, and practised, in my opinion his method is the simplest and best. It was in the winter of 1871 that I first saw Dr. Dew perform his method of artificial respiration, and I was so struck with its simplicity, ease and convenience to the operator, and its effectiveness, that I immediately adopted it and have practised it ever since.

Taking the child in your hands, as he has described, placing one hand under the buttocks and the thighs, and the other hand under the shoulders, and allowing the head and arms to fall backward and making an arch with the apex upward as in opisthotonos; and stretching the body backward, elongating and expanding the chest, thus making a forcible inspiration; then doubling the body up, shutting it up as it were, as in emprosthotonos, crowding the abdominal organs up against the diaphragm, you force the air out of the lungs. In making these movements it is so perceptible that inspiration and expiration take place that should there be any mucus in the nose or in the mouth, when you double the child up in the way described, you will see the mucus forced out, and if you allow the head and arms to fall backward you will see it drawn into the nose and mouth. After keeping up these movements for a short time you will have the respiration under perfect and complete control; and you can make these movements slowly or rapidly, just as you feel inclined, sitting, as it is not necessary to stand up; you can sit down in a chair and take it easy.

I fully believe I have saved a good many asphyxiated new-born infants by this method that otherwise would have been lost. You can also extend this method to older children, when they are not too large to handle. I was called to see a child one evening, a few hours old, who had been given by mistake a quarter of a grain of sulphate of morphia in solution intended for the mother. The child was cyanotic, thoroughly narcotized, with a sighing respiration at long intervals. I immediately resorted to Dr. Dew's method of artificial respiration. I arrived at the house at 7 in the evening. I used, of course, in addition hot water, flagellation, and friction, but I kept up the respiratory movements at intervals all night, and I may say that I attribute my success in saving that child to his

method of artificial respiration. The child is now a fine boy of sixteen. I cordially corroborate what Dr. Dew has said about his method and cheerfully recommend it. I will not take up more time of the Society, but emphasize that I consider it a simple, convenient, and most effective method.

DR. E. H. GRANDIN.—It gives me pleasure to take part in this discussion, because I can testify from clinical experience to the great value of the method which Dr. Dew has described this evening. Prior to last spring I was not familiar with this method. I thought that I knew them all, that I had tested them all, and had been dissatisfied with each and every one for one or another reason, particularly with Schultze's method, which I feared on account of the possibility of doing damage to the child. Last spring Dr. Dew described this method to me and asked me to test it clinically. It has been since then in use whenever opportunity presented, both at the New York Maternity Hospital and the New York Infant Asylum, and I am told by the house staff that it is the method *par excellence*. This is what we should expect. The method, as described graphically by the author, imitates best the natural method. When we are dealing with venous asphyxia, with a typically blue baby, this method enlarges the capacity of the thorax and then diminishes it. It exerts a suction effect on the great blood vessels which supply the lungs. In that way the right heart is relieved of its congestion. Not only are the thoracic muscles brought into play, but the diaphragm as well. In other words, the child is forced to breathe even as it would breathe were it not asphyxiated. For venous asphyxia, then, I do not know of any method superior to the one which Dr. Dew has described.

When face to face with the other and rare form of asphyxia, however, I hardly think it will alone suffice any more than the other methods which have been described in the books. The anæmic form of asphyxia is due, not to venous congestion, but to absolute poverty of the blood, and therefore in this rarer form, in addition to resorting to the method described by Dr. Dew, I should use the hot saline rectal injections which have, if I am not mistaken, been recommended by Dr. Jacobi. The object of these injections is, of course, obvious.

Next, as to the question of priority. In what I have to say I do not wish in any way to detract from the merit

due Dr. Dew. It is simply an exemplification of the saying that there is nothing new under the sun. A few days ago, in conversation with a gentleman of very large obstetric experience—Dr. Burnett, of Long Island City—this subject came up and I described Dr. Dew's method, and he told me that somewhere or other he had seen the method described, and had been in the habit of using it for years. This very evening, the first opportunity I had, I spent a little while in our library, and I found reference to this method, with cuts, graphically reproducing those Dr. Dew has shown this evening. The article was written by Dr. Harvey L. Byrd, of Baltimore, now deceased, formerly Professor of Obstetrics in the Baltimore Medical College, and was published in the *Baltimore Medical Journal*, 1870, being entitled "A Speedy Manner of Resuscitation in Asphyxia Neonatorum." By one year, therefore, Dr. Harvey L. Byrd had priority over Dr. Dew. Of course Dr. Dew was not familiar with it, and it was simply an instance of two men finding out the same thing at practically the same time. I do not wish, let me repeat, to be understood as detracting at all from the doctor's claim to priority, but I desire simply to give credit to a man who was considered in his day a leading obstetrician of the country, and who, being dead, cannot himself assert the claim.

As regards Schröder's method, I have never seen it described. There is a slight reference to it in one of the American translations of Schröder, but certainly in the leading works on obstetrics published in this country and in Europe no reference is made to it at all. And if any reference were made, we might as well emphasize the fact at once here that the method is as utterly different from that described by Dr. Byrd and Dr. Dew as black is from white.

Dr. W. T. Lusk.—I hope I shall not repeat anything which has been said in my absence. I was shown by Dr. Dew these photographs some three or four days ago, and have had opportunity to think them over; and I believe his method possesses more advantages than any of the three or four which are familiar to the profession. I think, however, he should not lose sight of the fact that there are no two cases exactly alike, and remember that a method suitable in one case may not be so well suited to another. I think Dr. Dew's method deserves the merit of being placed at the head of the list; but as to its being

one which should exclude all others I should question. We have to remember that in asphyxiated children we have in the first place to remove obstructions from the air passages, and, secondly, to restore the circulation. Where the air passages are filled with fluids aspiration is of great advantage, and no other method will so completely clear the infant's nasal passages as will mouth-to-mouth respiration. The pharynx should be then cleared with the finger. If the larynx and trachea have been invaded a small No. 7 English catheter should be passed through the vocal cords, and, after aspirating the mucus, breathing should be started by direct insufflation through the tube. When the heart has begun to beat perceptibly, then artificial respiratory movements should be tried. It may take a long while before spontaneous respiration is achieved. Meanwhile we have to effect changes in the circulation of the child. During the time of suspended animation the lungs are congested, all the cavities of the heart are overfilled with blood, the arteries are empty, and nearly all the blood is pumped over to the venous side. We may introduce air into the lungs to any extent we like, yet before life is restored to the child it is necessary to transfer the blood once more to the arterial vessels and restore the arterial tension. For that purpose there are obvious advantages in the method which has been recommended by Dr. Dew.

In the method of Schultze, which I know best, as we swing the child upward the diaphragm is forced upward by the pressure of the intestines and abdominal viscera, the lungs are compressed and there is forcible expiration, all the air in the lungs and any mucus that may be in the upper air passages are driven out, and with greater force than by any other method. That is its value. But, besides that, by the swinging movements the blood is forced from the left ventricle into the aorta, and a space made for the blood in the left auricle to flow down into the left ventricle. At the same time blood is forced from the right auricle into the right ventricle. Then, if we allow the child to swing downward, inspiration is produced and the blood passes through the lungs from one side of the heart to the other, room having been made for the circuit by the previous emptying of the left ventricle. From the left ventricle the blood passes into the aorta. By an alternation of these movements we have started the circulation perhaps more effectually than by any other

method. What Dr. Dew has aptly called the pump-like action is produced. But there is one drawback to it—that is, if the child is feeble these movements are too severe.

I do not believe that anybody of experience could possibly injure the liver or spleen by the Schultze method. I have used it a great deal. There may have been something else in the cases of injury reported besides the mere movements which produced detachment of these organs. But the objection to the method is that if the heart's action is very feeble and the swinging movements are repeated, in a brief period of time the heart will cease to beat altogether and life become extinct. It is therefore of much importance that we should have a method like that of Dr. Dew, which disturbs the child but slightly. Formerly I resorted to Sylvester's method, and from time to time reinforced it by Schultze's, but now I am sure I shall try the method of Dr. Dew; and I do not believe it will do any harm once in a while to give the child two or three swings, first for the purpose of forcibly ejecting mucus from the air passages, and, second, because I believe it gives an impetus to the heart, when once the heart's action is started, which cannot be accomplished as well by any other method.

Dr. W. E. Forest.—After this unanimity of commendation of this method, it may seem a little presumptuous for me to say that I think it is not a very good method. In the first place, there has been nothing to show yet that by this method one cubic inch of air can be drawn into the lungs. Now, bear in mind it is a very important point that when a child is born asphyxiated it is born with the lungs absolutely collapsed, and you cannot force air out of the lungs until you get it in, and the first thing to do must be to get air into the lungs. Now, with your permission, I think I can show this (on the cadaver). When that child is bent back it is supposed that you have drawn air into the lungs. What have you in reality done? You have simply pushed—remember, the lungs being collapsed—you have simply pushed the spine up toward the thorax; the rectus abdominis muscle running from the sternum and the ribs down to the pubes is stretched, and the ribs, instead of being raised, as they are when the arms are drawn up, are pulled down. You have pushed the spine toward the ribs; you have pulled the ribs toward the spine. Instead of elongating the antero-posterior and lateral diameters of the thorax, you

have shortened them. To a certain extent, we will admit, the diaphragm descends, but to a very small extent. Now, has there been an experiment to show that by that movement there is a single cubic inch of air, or half a cubic inch of air, drawn into the lungs? Champneys' experiment, on the contrary, very effectually proved that no air is drawn into the lungs by that movement, and I have repeated his experiments time and again, and I have failed to draw a particle of air in by that inspiratory method. And I fail to see how it can be done. When you are pushing the spine toward the ribs and drawing the ribs toward the spine by the rectus abdominis, how can the antero-posterior or lateral diameter of the thorax be increased? If you take a rubber tube and simply bend it on itself, the parts in front would separate from each other, but the tube diameters would not be increased. Now, if you cannot get air into the lungs, how can you force it out? The first step in artificial respiration is to get air into the lungs. It is the most important step of all. The expiratory movement (Dr. Dew's) is very good, it is excellent, it could not be better. But it is useless unless you have put air into the lungs. A child after it has breathed and died, or a child which has been poisoned or drowned, can be resuscitated and the respiration be kept up by this method. But a child born asphyxiated, with collapsed air cells, there being no resiliency, cannot be resuscitated, in my opinion, by this method, and I have tried it time and again. That is the whole point, the whole failure of this method, and it is a fatal failure, and has been proved by definite experiment to be useless; and no experiments have been shown here to contradict such evidence, except that gentlemen claim to have resuscitated children by it. But that is a very indefinite statement, for a child apparently born dead can be resuscitated by a good many methods. But when you get a child whose lungs must be artificially ventilated, I claim it cannot be resuscitated by this method. I do not want to take up much time, but I will say a word about a method which I have originated.

Nothing has been said about direct insufflation of air in Dr. Dew's method. To blow air into the lungs is, according to the best authorities, of first importance. My own method is to place the child in a sitting posture in a bath of hot water, the water rising up above the child's breast; this serves to keep up the child's vital heat and is

a very important point. The child's head is supported between the operator's thumb and forefinger of the left hand and falls backward sharply, which throws the cervical vertebræ forward and draws the larynx backward, thus compressing the flaccid œsophagus between the cervical vertebræ and the larynx. This effectually closes the œsophagus and prevents air, when blown into the mouth, from entering the stomach. The operator next takes the child's hands, palms to the front, in his right hand, and carries them straight up over the child's head until its body is lifted a little from its seat and hangs suspended by its arms. This puts the muscles running from the shoulders to the ribs on a stretch, and thus the ribs are drawn up and the antero-posterior and lateral diameters of the thorax are lengthened; the diaphragm is ready to fall also and thus increase the vertical diameter of the thorax. Now, with the child in that position, and the œsophagus closed and the trachea open, you lean forward, place your mouth over the child's, and blow directly into the child's lungs. That is inspiration, and it is inspiration which will open the collapsed air cells in the lungs. In the second step you lower the child, bring the arms down, bend the child forward, press with one hand behind and one in front against the thorax, throwing the ribs back toward the vertebræ and expelling the air. At the same time that the child is bent forward the abdominal organs are crowded up against the diaphragm, and you have the three diameters of the thorax lessened exactly as you have in Dr. Dew's expiratory movement.

I still claim, by absolutely direct experiments made by Champneys and myself, that this method (Dr. Dew's) cannot bring air into the collapsed air cells, and therefore I do not believe it is a good method where the child is badly asphyxiated.

Dr. J. C. Edgar.—The ground has been so fully covered by the preceding speakers that there appears to be very little left to say concerning asphyxia neonatorum.

At the same time I desire to thank Dr. Dew for so graphically describing this method and comparing it with others. There is one point I did not understand Dr. Dew to bring out, which in conversation with him some days since he spoke of, and that was the advantage of this method in that it could be readily used between the thighs of the mother before tying the cord in those instances of asphyxia neonatorum termed pallida, where we

delay separation of the cord for the purpose of securing the last half-ounce or ounce of blood obtainable.

The method, Mr. President, is undoubtedly a good one because both an easy and a safe method. There is no question about that. As far back as 1870 it was termed the "ready method" because of its simplicity.

The method is not, however, altogether a new one, as has been claimed for it here this evening. Dr. Harvey L. Byrd, in the *Baltimore Medical Journal* for November, 1870, and again in the American supplement of the *Obstetric Journal of Great Britain and Ireland*, vol. i., describes and illustrates a method of artificial respiration which is practically identical with the one demonstrated here to-night, as any one may see by glancing at Byrd's illustrations.

For five years I have taught Byrd's method, and used it both in private and hospital practice, and I have always told my students it was a good method and an efficient one, but that it almost always failed as regards the inspiratory posture in those cases of advanced asphyxia where there are no spontaneous efforts on the part of the new-born child at respiration. As Dr. Forest has shown us this evening, the fault lies in the inspiratory posture. The expiratory posture, experience has proved to me, is all that may be desired.

Given an apparently still-born child with collapsed lungs and no spontaneous efforts at respiration, and the inspiratory posture of Drs. Byrd and Dew will in most cases prove insufficient. On the contrary, where spontaneous efforts at respiration are present the method works beautifully and is a good one.

No inspiratory posture has given me such satisfaction as that of Schultze. With it collapsed lungs may be inflated whether the child be alive or dead. Experiments have proven that the lungs of a still-born child, that have never been ventilated, may be made to float by the method of Schultze. What more can we desire? The expiratory posture of any of the accepted methods will prove efficacious if we make sure to clear the respiratory passages of mucus and foreign substances. In regard to the danger of the Schultze method, I know that there is a case of fractured clavicle in literature due to the method, and it has been claimed that it also produces pleural rupture, hæmorrhages into the viscera and anterior mediastinum. So will any method improperly applied.

The objection has been raised to the Schultze method that it requires too much room to perform it. My experience in some of the smallest attics and cellars of the tenement houses of the East Side has been that wherever there is room for the operator to stand upright, there can he properly swing the child. Intelligently used, as Dr. Lusk has said, I believe the Schultze method as efficient as any of the others.

There is this difference, Mr. President, between the methods of Drs. Byrd and Dew, and it is that the method of grasping the child as shown to-night by Dr. Dew is infinitely better than that of Dr. Byrd.

Dr. D. H. Stewart.—There seems to be some doubt as to what Schröder's method is. With Dr. Dew's permission I will demonstrate it on his cadaver. There is nothing new about it. It was described as early as 1868, You take the child in either hand, right or left (Schröder, I believe, begins with the left), lay it on the back in your hand, the legs and arms hanging down and the chest as in opisthotonos, and that is the position of inspiration; then on its chest and stomach on the other hand—emprosthotonos—and that is the position of expiration. You thus keep on reversing the child, changing from hand to hand. That, at least, is the way it was explained to me in Germany last summer. It is a valueless thing, I believe.

Dr. E. A. Ayers.—I shall only speak for a moment on one point. I have practised Dr. Dew's method six or seven years, and was led to adopt it by a consideration of the fact that, as it seems to me, one of the most important things is to preserve the animal heat of the child. The resuscitation of the new-born child takes considerable time, and during that time it may practically lose heat enough to kill it. The only method that I could employ to preserve this heat was the method described by Dr. Dew, which allowed me to immerse the child's body in hot water. It is my custom to have two large bowls in such cases, one of cold water, the other of hot, as hot as the hands can conveniently bear, and perform this method with the child's body all under water, holding simply its mouth out, and taking it out and dipping it suddenly and quickly into cold water to get the stimulus of the change of temperature. As far as I am able to observe, this is most eminently satisfactory. The longest case which I have resuscitated took thirty-eight minutes, during which time artificial respiration was kept up by

this method. I do not believe I could possibly have saved that child by any method which left out hot water. As to the efficiency of Dr. Dew's method, I must confess that I cannot agree with Dr. Forest's objections, for I have seen its value too clearly demonstrated to doubt it. The noise of inspiration, the noise of expiration, have been clearly evidenced to me time and again. I have used it a good many times. It is about the only method I do use, and I think Dr. Edgar's suggestion that it can be employed before the cord has been cut is an excellent one, too. Take it all in all, it is true that the different methods have their special value. Schultze's particularly, as Dr. Lusk has said, is more efficient for clearing out the throat than any other; but we must consider the most essential points, and to me it seems this method (Dew's) is the only one which will enable one to keep up artificial heat and thus help in the great majority of cases in restoring the child.

THE PRESIDENT remarked that it had been shown Dr. Byrd's method was not identical with Dr. Dew's.

DR. W. A. EWING wrote as follows: I shall be unable to be present at the meeting of the Academy of Medicine when you read your paper on your method of resuscitation of the asphyxiated infant, and I have only time to write a line expressive of my hearty approval of that method, which I remember to have seen practised by you more than twenty years ago, since which time I have practised it frequently, as you know, and always with success, when success could be obtained by any method.

DR. J. D. BRYANT sent the following communication:

Mr. President:

The determination whether or not the manipulations advised by Dr. Dew cause an interchange of the air contained within the lungs in cases of the kind to which the manipulation is applicable, seems to me to be based upon the fact of whether or not these manipulations cause increase and diminution of the thoracic cavity or its contents. That increase does take place in the circumference of the chest during extension of the spine of the dead infant there can be no question, as a careful observation as to the measurement before and after this movement demonstrates the fact; however, the differences in measurement are slight indeed. It seems to me that this increase is due largely to the traction exercised on the clavicle, sternum, first and second ribs, by the scaleni and sterno-

cleido-mastoid muscles. The bending of the neck backward, otherwise known as hyperextension, is the cause of this muscular influence on the chest, in my opinion. While it is true that extension of the spine may exercise some degree of influence in increasing the antero-posterior capacity of the thorax, still the influence exercised by this movement on this portion of the spinal column must be limited indeed, as the ability to extend this part is comparatively slight owing to the spinous processes that overlap each other so closely. In fact, one can do scarcely more than straighten the dorsal curve when it is attempted under the most favorable circumstances. To emphasize the wisdom of this impossibility, one has but to recollect that the dorsal vertebræ afford attachment, directly or indirectly, to important thoracic viscera, the disturbance of which by free movements of the dorsal vertebræ would seriously modify their functions. I do not think any consideration can be attached to the change of the dimensions of the thoracic cavity, dependent alone on the straightening of the dorsal spine. During the movement of flexion of the body in this method the abdominal viscera are forced upward against the diaphragm, which in turn makes a similar pressure on the superimposed thoracic viscera. The effect of this, of course, is evident without further comment. Air is expelled by this force through the trachea. Consequently one observes that by the alternate repetition of these acts air is caused to escape and enter the thorax through the same channel. If the chest of the adult be tightly surrounded by a tape measure, and the adult be caused to lean backward so as to extend as much as possible the dorsal and cervical portions of the spine, the circumference of the chest wall is markedly increased by this act, and therefore, as the result of it, additional air enters the thoracic cavity.

If, however, the cervical spine be flexed before extension of the dorsal spine, the effect of dorsal extension on the increased capacity of the chest is much less noticeable. If now, during extension of the dorsal spine, the flexed cervical spine be also strongly extended, a marked change in the capacity of the chest is noticeable, attended at once by the sensation indicative of the entrance of air to the lungs. The differences of sensation and capacity, as modified by the different positions of the spine, seem to me to depend almost exclusively on the action

of the scaleni and sterno-cleido-mastoid muscles upon the upper bony structures of the thorax. In this respect, of course, the influence is similar to that produced by extension of the dead infant's body, attendant on the manipulations distinctive of the method under consideration, modified, of course, by the fact that the muscles in the one instance are living tissue and in the other dead. In the one instance the muscular tone shortens their leverage and increases correspondingly their power of action on the chest, while in the other, the dead, these influences are entirely absent. I regret, indeed, that time has not permitted me to think more carefully regarding this matter, yet from my cursory examination I am fully convinced of the following facts:

1st. That extension of the dorsal and cervical spines in the living or in the dead subject increases the capacity of the thorax, and therefore causes the entrance of air into that cavity, other things being equal.

2d. Flexion of the spine and the forcing of the abdominal contents against the diaphragm and thoracic viscera diminish the capacity of its space, and therefore cause expulsion of air through the trachea. The same movement, too, no doubt stimulates the action of the heart by the direct contact with it of unusual pressure agencies.

3d. Whether or not this method of procedure be better than others of greater repute in a practical sense, is a matter which can be better decided by those who are constantly employing artificial respiration in their everyday experience than by myself.

DR. J. H. DEW.—I have very little more to say, except that I have practised this method almost exclusively, and that what Dr. Ayers has just said is very true. I did not look very much into the *rationale* of this peculiar method of artificial respiration for a long time after I had begun to practise it, but clinical experience will prove, notwithstanding the objections which have been offered by Dr. Forest, that air is unquestionably sucked in and forced out. I only ask the gentlemen to try for themselves, if they wish to see it fairly demonstrated. We may theorize as much as we please, but practical experience at the bedside is worth more than all the theories. There are a great many practical things we do for the sick which are most useful, and yet it is very difficult to explain their exact action. There is unquestionably a combination of conditions which brings about the good results that I and every man here

who has practised this method of artificial respiration have observed. There are several of us—Dr. Ewing, Dr. Thompson, and myself—who have practised it for twenty years, almost exclusively. Many cases of asphyxia occur which we all know it is impossible to resuscitate, do what we may; it matters not what method we adopt, we cannot resuscitate them. Each method, I have no doubt, has certain features to recommend it. But I think, as one for immediate use, this method has great advantages; indeed, I believe it has many advantages over all others. A great many children are born more or less asphyxiated. In such cases we invariably proceed at once to excite the reflex stimulus, but if we do not establish respiration by that means we have to resort to artificial respiration. Hence some ready, prompt method, that can be resorted to instantly and be easily performed, is extremely desirable, and I believe that, nine times out of ten, this method will answer all purposes. In extreme cases any or all methods may fail. The same is true in the practice of both medicine and surgery for the relief of disease. Do what we may, patients will sometimes die, and the same is the case in asphyxia neonatorum.

Stated Meeting, February 16th, 1893.

PRESENTATION OF GOSSELIN MEDALS AND FAIRFAX FORMULARY.

By F. A. CASTLE, M.D.

I HAVE the pleasure of presenting to the Academy, in behalf of Mr. Samuel P. Avery, this frame containing two bronze medals. They are reproductions of the artist's study for larger tablets erected as memorials of the late Léon Gosselin in the École de Médecine, in Paris. They are the work of one of the most accomplished of French medallists, and are duplicates of those in the Metropolitan Museum of Art; and the Fellows may be proud of their acquisition, both as a work of art and as a memorial of an eminent surgeon and teacher.

Mr. Avery has asked me to offer to the Academy also this interesting and valuable book. It is one of a small edition published in 1890 in *fac-simile* of a manuscript nearly three centuries old, with an introductory essay. The original was used, and largely written, by members of the Fairfax family in England, and consists in greater part of medical formulas, household recipes, and matters relating to domestic practice. It will interest Fellows of the Academy, not only by the quaint nature of its contents and its antiquity, but also through the fact that Lord Thomas Fairfax came to this country, having inherited from his mother (daughter of Lord Culpeper, the colonial governor) six million acres of land in Virginia; that George Washington, when but sixteen years old, surveyed much of this estate; that the Washingtons and Fairfaxes were related by marriage; and that the present representative of the family, entitled to be known as the eleventh Lord

Fairfax, is Doctor John Contee Fairfax, of Bladensburg, Md., formerly a practising physician.

Another feature of the book has impressed me. It will be remembered that Wilkie Collins, in his novel, "The Woman in White," speaks of the sign ℞ used in prescription-writing as a relic of mythology and as representing an invocation of the aid of Jove. A comparison of the chirography of portions of this ancient manuscript with that of many prescriptions written at the present time, will suggest the thought that the illegible character of many of the latter is likewise a relic of the past.

The artistic features represented by these two gifts that interest physicians afford an occasion for more extended comment which is most tempting. They are features which have received too little attention in the work of the Academy during recent years, and in the changes which occurred ten years since we lost the active cooperation of some whose tastes were cultivated in such directions. With the present conditions of prosperity one may expect a revival of interest in the Academy for matters of such artistic and historical nature, and I hope that these gifts by Mr. Avery may serve to inaugurate it.

The Academy is already indebted to Mr. Avery by former gifts of this nature, and I therefore move that:

The Recording Secretary be directed to express to Mr. Avery the thanks of the Academy for these very acceptable gifts and for his continued interest in its welfare.

Carried.

THE APPLIED PHYSICS OF PHYSICAL DIAGNOSIS.

FIRST PAPER.—ACOUSTICS.

By CHARLES E. QUIMBY, A.M., M.D.

It was Talleyrand, if I remember aright, who said that "Language was given to man to conceal his thoughts."

There is a minor grade of hope, coupled with the discouragement of this statement, in the knowledge that language is capable of subserving any exact purpose, which becomes major when we recognize that admitted power to conceal involves a potentiality at least of revelation. But the sting of the sarcasm lies in the truth of the implication that what should be a Damascus blade for intellectual conflict is made a barbaric boomerang by mental apathy. As an indication of the possibilities when words are used with attempted precision, the statement is, perhaps, sufficiently suggestive of controversial difficulties; but for random discussion with indefinite terms it is impotent even to suggest the resulting mental chaos.

Among the host of accommodating terms which we employ with a carelessness fully justifying any degree of elastic significance, probably none convey more vague ideas than those used to designate so-called physical signs. This fact becomes oppressively patent in attempts to communicate personal auditory impressions of the sounds produced in any given chest. Nor do we achieve a conspicuous success in our efforts to extract consolation from the consciousness that our verbal pictures are no more ragged in their outlines than are the conceptions they so vainly struggle to depict. Our "confusion" the rather becomes "worse confounded" as we appreciate that inability to give definitions arises from lack of possession. Yet this knowledge were cheap at any cost, since, in disclosing the truth that a definition is not words, but *an intellectual differentiation of one entity from all others*, it removes every doubt as to the fundamental cause of our present difficulty.

That the nomenclature of physical diagnosis is at present in a condition of most annoying confusion was conclusively proven at a meeting of the Section on General Medicine of the Academy, held in January, 1892, when, with cases presented to determine just this point, it was the exception that any two of the gentlemen present, among whom were teachers of physical diagnosis, used

the same term to designate any given respiratory sound, notwithstanding the comprehensive character of these terms. A leading work on this subject states definitely that "rude respiration embraces every modification between normal vesicular and bronchial respiration." Moreover, that descriptions of the physical phenomena, designated by current terms, contain so many words of relative significance as to prevent any definite value, is pertinently illustrated by this description of bronchial breathing; "The inspiration is of tubular quality, of higher pitch, of marked intensity, and of considerable duration. The expiration is of tubular quality, of higher pitch, of greater intensity, and of longer duration than inspiration." Here every definitive word is comparative, and the only standard given for valuation is the equally indefinite term "low" in the preceding paragraph. Simple justice demands acceptance of my statement that this and similar quotations are not made in criticism, but are used to demonstrate the indefiniteness of the thing described, solely because the absolute accuracy of the description gives it unequalled value as illustrative and conclusive proof.

It requires no special acumen to appreciate the forces productive of these conditions, or to see them as natural, if not inevitable, misfortunes, for the history of physical diagnosis affords an obvious as well as instructive illustration of the development of most practical sciences.

In the earlier stages, when facts are received on sufferance instead of being sought for, only gross phenomena and superficial relations are recognized, while logical analysis is absent or sporadic and tentative.

Following this comes a pre-eminently analytical period. The transition is frequently abrupt, and the process often pushed to such unjustified extremes, possibly even by illegitimate methods, as to provoke equally intense reaction with indiscriminate rejection of truth as well as error. When developed under the restraints of judicial conservatism, however, the returns in scientific

accuracy, definite methods, and permanent results are of inestimable value. Yet the peculiar value of analysis involves a proportionate danger. Increasing technical skill, which is the inevitable result of constant repetition, begets impatience of details by minimizing their personal value, and initiates a progressive tendency to more comprehensive and rapid methods; to deal with products rather than processes, until finally the latter are abandoned entirely.

In this evolution, habit, or thoughtless failure to appreciate the change, may cause retention of terms and motions long after the processes they represent have become empty, deceptive forms.

It is essentially this position which physical diagnosis appears to occupy at present. We recognize products and call them processes—reasoning by results in place of relations. We have accepted dicta instead of demonstration—regarding "when" as "why," and honesty as proof of accuracy. We cling to our terminology in perfect good faith, never suspecting that our symbols have become cymbals. The effect upon methods of instruction was inevitable, and developed with a certainty and rapidity directly proportionate to the skill of the teachers. Gradually and unconsciously the most complex products became, to them, so familiar as to forbid a just appreciation of their bewildering intricacy to the eyes of ignorance. Final results are manifested by the prevailing demonstrative methods of teaching physical diagnosis. It was in accordance with these methods that, now some six years ago, I first essayed to teach this subject. For a time faith magnified meagre results to acceptable, because deceptive, proportions. But the crucial test of teaching soon forced an unqualified admission of failure, and disclosed the cause in personal ignorance. The consequent study which aimed to remove this cause was based upon the following directive and definite statement, found in Prof. Loomis' work on "Physical Diagnosis":[1]

[1] Edition of 1887, p. 23.

"To estimate the value of percussion and to understand its true significance, you must first learn *to appreciate correctly the elements of sound.* . . . Those elements or acoustic properties of percussion sounds which concern us clinically are termed, respectively, *Intensity*, *Pitch*, *Quality*, and *Duration*, of which *Pitch* ranks first in importance."

The term "physical diagnosis" strictly implies only determination of the physical properties of the body tissues, and, although extended by common consent to include recognition of specific causes of such properties, is still restricted to those cases in which the physical factor maintains at least a superficial prominence. Thus, clinically, the product so universally recognized by this term involves the combination of three sharply defined factors. These factors are:

1. Accurate knowledge of the anatomy, physiology, and pathology of the tissues in question, and of the principles of physics involved in such investigations.

2. Possession of the requisite technical skill, and its application in making the first factor specific for any given case.

These two factors are sufficient for a purely physical diagnosis. They give definite and absolute knowledge of physical conditions, and of these only.

It is the third factor alone which transforms these lifeless facts to most potential measures for the recognition of disease.

This factor is the intellectual estimation of the diagnostic significance of a pure physical diagnosis, as proof of specific disease, from its relations to all associated or precedent symptoms and conditions. As factors in a *clinical* physical diagnosis, the first is solely a question of accurate information and memory; the second, of trained mechanical skill and quickened sensory acumen.

The third is a question of *brains* and *the man*, which will dominate in the future, as it has in the past, the relative value of diagnosticians. To the man of genius, who

deals with totals, the first factor may be of minor importance. For any who may have mislaid their genius or neglected to make necessary repairs, this attempt to restore the factor of pure science to its legitimate position in diagnosis, and render it available at its full value for both teaching and practice, may not be entirely devoid of interest. To prevent repetition and delay, permit me to offer any standard work on physics as authority for all statements of physical principles or facts, whether quoted formally, in substance, or employed simply by implication. My own references have been largely to Ganot, Atkinson's translation.

Physical analysis of sound, using this term in its exact significance of *auditory sensations*, has shown that the ultimate sound unit is the result of isochronous vibrations of matter, manifesting but three phenomena, each of which produces a characteristic sensory impression. These material vibratory phenomena and their sensory indications, known as the elements of sound, are: 1. Rapidity of vibrations, determining pitch. 2. Amplitude or extent of vibrations, fixing initial intensity or loudness. 3. Continuance, the primary cause of length of sound.

While this ultimate sound unit of physics, caused by a single set of vibrations with three inseparable phenomena, and therefore presenting only the elements of pitch, intensity, and duration, must never be lost sight of as the basis of all sounds, it is equally important to remember that the unit of applied acoustics is determined solely by the ear, and that this unit, even with the maximum limit of auditory acumen, is, of necessity, a complex product, since the relations and properties of matter render the development of multiple sets of sonorous vibrations an inevitable accompaniment of the excitation of one such set. The physical demonstration, by which all auditory impressions are shown to be multiple, discloses limitations to our powers of auditory analysis that instantly assume a position of fundamental importance, as determining the

sole clinical standard for estimating the composition and elements of sound.

It will be useless for any one to read further who does not clearly recognize the significance of this relation as the point of contact between pure and applied acoustics, and appreciate that the transition to the latter involves the introduction of a variable personal factor, and its constant presence in every problem of applied acoustics.

The general character of these limitations may be illustrated by drawing a pencil across the teeth of a comb, slowly at first, and then with increasing rapidity, until what at first are distinctly separate strokes become merged into one rough sound with distinct pitch. They are the direct analogue of our ocular limitations, as demonstrated by Newton's disc. We thus have an auditory unit for clinical work, determined negatively rather than positively; subjective, not objective; dependent upon reception and perception; absolutely independent of inception; but a most fruitful source of deception; a unit which derives an especial and unique value from absolute independence of the origin of its own cause. Yet it is just this indefinite, fluctuating unit, in which danger and value are each the cause of the other, this unit of *effect*, not *cause*, which we are compelled to use to measure sums of *cause*. Much confusion and many errors have resulted from failure to recognize this relation, and the consequent necessity that any sonorous unit which contains terms of multiple origin, and depends for its singularity upon fluctuating auditory acumen, cannot, *per se*, possess any fixed elements.

While all manifestations of elements, as properties of complex sounds, are thus made possible only by our inability to separate the terms, these elements are nevertheless determined solely by ratios between the component terms. Consideration of these elements as auditory sensations, in connection with their causative phenomena, will show that pitch is, essentially, the only qualitative property peculiar to sound, and that intensity and dura-

tion are practically qualitative properties of pitch, although of independent causation. Recognition of these double relations enables us to appreciate more readily the physical demonstration that pitch, as an element of a sonorous sum, is derived from the term of greatest intensity. Moreover, since the remaining terms, as sound units of distinct pitch, are evidently not nullified, it should have been obvious that each term develops its special sensory impression, aside from any modifying effect it may have upon the dominant term. Nevertheless it required Helmholtz's brilliant experiments to demonstrate our recognition of these associated sensations in the element designated quality, and to make plain the peculiar significance of this element as a phenomenon *solely of composite sounds*, and the *sole inherent element* of such sounds, the element characteristic of sensory limitations, when compared with the three fundamental elements, as representing inseparable phenomena of physical sonorous vibratory units.

This relation has received comparatively but little consideration even from physicists, because the practical application of acoustics has been confined almost entirely to the art of music. The utterly opposite character of that art compared with this of physical diagnosis is clearly shown by the fact that in music the problem is one of sound *production*, while in physical diagnosis it is one of sound *interpretation*. In the former the objective point is sensual gratification. Therefore, with conditions of sound production under control, predetermined results are attained and made certain by elimination of all uncertain and uncontrollable conditions. In the latter the value of results is due, pre-eminently, to the uncontrollable character of the sound production and the conducting media. Helmholtz's original analysis, which established the multiple character of all auditory impressions, coincidently proves that the same forces impose fixed ratios, numerical as well as elemental, upon sonorous terms of identical origin—*i.e.*, in a single sonorous body. Qual-

ity, then, as an element of sounds of single origin, is, plainly, as fixed and definite as pitch. The impossibility of any such constant relations in sounds of multiple origin is no less obvious than that singularity of sounds, when determined by auditory standards, has no necessary relations to the origin of component terms. Sound units thus separate naturally into two classes as composed of terms, 1, Of mutually dependent origin in a single sonorous body; 2, Of multiple, and therefore independent, but not unrelated, origin. I desire to call special attention to the fact that in the first class each sonorous term possesses only the elements pitch, intensity, duration, and does *not* possess quality; while in the second class, which includes most thoracic sounds, quality of the auditory unit is derived from terms each of which presents quality and pitch in a fixed ratio. Therefore, since all the elements of any thoracic sound of multiple origin depend upon term ratios, for every change of pitch in such sounds quality must undergo coincident and proportionate change. Pitch, which is susceptible of exact measurement and record, thus may become an equally exact measure of quality. This principle is of the utmost value in physical diagnosis, yet, so far as I can learn, it has never before been recognized or appreciated. Its application, moreover, is not limited to respiratory sounds, but is equally available in localizing râles or estimating any tissue changes modifying pitch of thoracic sounds.

To attain sharper distinctions, and consequently greater accuracy, it becomes necessary to indicate the significance attached to certain terms. Hereafter, therefore, the term *pure*, as modifying sound, will define the ultimate physical sonorous unit from a single set of vibrations, possessing pitch, intensity, and duration, but not quality. Similarly, *single* will define the auditory unit of single origin, in which the terms are pure sounds producing a pitch and quality of fixed ratio. *Composite* will describe the auditory unit in which the terms are single sounds; while *compound* will indicate a sonorous sum which can be

heard at will as one or more composite sounds. The power of auditory analysis, implied in this definition, is of the utmost importance. Nor is it possible to ignore the fact that transition from composite to compound sounds, or *vice versa*, is dependent upon auditory analysis, and that, under proper training, one's capacity in this direction may be so developed as to extend the limits, within which singularity or multiplicity of sounds is subject to the will, to an exceedingly valuable degree as measured by clinical results. For some years, even before the present study was undertaken, it had been a matter of extreme personal interest to see how rapidly skill in this direction may be increased, as proven by the conscious separation of one instrument from all others in an orchestra. My ears, however, do not yet disdain ocular assistance in disentangling the oboë and bassoon. This numerical analysis of sounds is, so far as I am aware, not generally employed. It nevertheless affords many most valuable indications of pathological changes not otherwise obtained.

The foregoing physical principles and relations are the basis of the following propositions in applied pulmonary physics:

1. When composite sounds are employed as exponents of their material source, the first differentiation should be numerical, as between a composite or compound sound.

2. Since the distinctive elements of pitch and quality in composite sounds are dependent upon ratios of intensity between terms, their full significance can be estimated only after determination of the relative value of the two factors of intensity, *i.e.*, initial intensity and transmitting media. (Illustration: Weakness of respiratory sounds with increased vocal resonance must depend on initial intensity. Weakness of respiration with diminished vocal resonance must depend in part on transmission.)

3. Although pitch of a composite sound cannot change

without change of quality, the reverse is not true. On the contrary, every conceivable combination of terms with an attendant new quality may be developed without change in pitch of the composite sound, so long as one term maintains a dominant intensity.

I am convinced that failure to recognize this fact has been a prolific source of error in physical diagnosis. The error has been, primarily, in assigning a value to pitch in *composite* sounds upon the basis of pitch values in *single* sounds. Notwithstanding the liability to error from this cause, the peculiar value of pitch in clinical work is largely dependent upon the composite nature of the sounds considered; it even acquires a double value, since by persistence as an element of single sounds it is an index of local conditions at the point contributing that term, and, as an element of composite sounds, it serves to measure variations in transmitting media.

Demonstration of these propositions in connection with percussion and auscultatory sounds, as related to pulmonary disease, presents the following problems:

1. To determine their composition and the practical or desirable limits of analysis.

2. To ascertain the origin of the terms, as defined by the primary analysis, and their general and specific relations in determining elements of composite sounds.

3. To establish rules for estimating absolute and relative values of elements in composite sounds as indices of tissue changes.

4. To define the relations of physically defined composite sounds, as associated terms, to current descriptions of these sounds, for the purpose of establishing accurate definitions.

5. To ascertain the degree of skill required to make practical use of pure physics in physical diagnosis.

The present paper is limited to a consideration of composition and general pitch relations.

An examination of current works on physical diagnosis justifies the statement that "percussion sound," "percus-

sion tone," "percussion resonance," and "percussion note" are used as essentially synonymous. In no case have I found any suggestion of more than one sound possessing diagnostic significance in the percussion product. All authorities speak of variations in pitch of "percussion resonance," in terms that necessitate the existence of such relations between resonance pitch and tissue changes, as are definitely indicated, in some cases, by the specific statement "that pitch of percussion resonance varies directly with the volume of pulmonary air."

The fact that resonance, as a sound of peculiar origin, is so frequently a component term of composite sounds, imparting a characteristic quality necessarily defined as "resonant," has obscured, if not destroyed, the distinction between the substantive and its adjective. Confusion on this point has been increased, if not directly caused, by the failure on the part of physicists to make ultimate, or, perhaps more exactly, sufficient, definitions. Even in so recent a work as the "Century Dictionary" "resonance" is made to include these three distinctly different acoustic phenomena: 1. The sonorous confusion and intensification caused by multiple reflections of sound within definite limitations of distance; 2. Vibrations in unison with, and reflection of, a precedent sound, by a solid body; and 3. Unison vibration of definite amounts of partially confined air. This definition, moreover, is essentially in accord with the statements of physics. My objection is not to the use of the term to indicate every form of resounding, but to its unmodified application to either form of resonance as convenience may direct. In the first case its application is entirely indefensible as a scientific term, since the indicated phenomenon is purely a reflection of existing sonorous waves, and is not the initiation of new vibrations. Moreover, it is, admittedly, a question of distance only when resonance becomes echo. In the second case, also, reflection plays quite as prominent a part as does strict resonance.

The third form alone is properly called resonance, since

it is the one universally understood in scientific discussions. Its peculiar significance and value are derived from limitations of the resounding to a single sound, determined by the resonant cavity, independently of the primary sound as regards pitch and quality. Acceptance of the multiple significance of "resonance," in common usage, cannot disguise the fact that in physical diagnosis it is universally accepted as due to atmospheric vibrations in the pulmonary air spaces. As a vibratory result, then, of the percussion blow, it is initiated by a transmitted motion. This motion, therefore, as the result of the same blow, must represent material sonorous vibrations, coincident with resonance throughout; for they are not simply precedent to resonance, but subsequent as well, since the resonance vibrations reach our ear only after passage through the chest wall, in which passage is equivalent to vibration of the conducting media. We are offered but two horns to the dilemma. If the sound from the thoracic wall is no part of percussion sound, as commonly understood, it is imperative that we hear the resonance entirely separate from this sound. That is, we must admit and clearly differentiate two sounds as the result of percussion.

This certainly is not done. On the other hand, since we must admit that every percussion blow excites sonorous vibrations in two distinct tissues, the thoracic wall and pulmonary air, to define the resulting sound as single, and yet ignore the influence upon that total of the sound precedent to, and causative of, the accepted portion, when such causative vibrations are clearly of independent origin, is simply impossible in a scientific discussion. This thoracic sound, therefore, demands recognition and valuation, as the primary vibratory cause of resonance and a modifying factor in resonance transmission. Investigation of this sound from the physical standpoint, in connection with its relations to the thoracic contents and tissue changes, justifies such demand and establishes its position as the major term of the percussion product. It therefore requires a definitive name.

Note is simply numerical; sound is generic and would require a descriptive adjective. Tone is not, at present, detailed for special duty, and is sufficiently appropriate as it defines a sound of distinct pitch and implies further differentiation by quality. With the previous definition of percussion resonance accepted, the entire sonorous product of pulmonary percussion becomes a dual compound sound, containing the terms "percussion tone" and "percussion resonance." Clinically, percussion tone is the exponent of solid tissues, and percussion resonance of pulmonary air spaces.

I have not found any practical consideration of the composition of percussion resonance. Accepted statements are limited to defining the cause, direction, and degree of variations in resonance pitch, and are based, first, upon the definite physical demonstration that resonance pitch is fixed by the resonant cavity, and, under given conditions of atmospheric tension, is not subject to modification; second, upon the tacit assumption that the pulmonary air space acts as a single resonator. Demonstration of the truth of this assumption is proof of the accuracy of accepted statements.

But physical investigations have shown that the boundaries of resonant cavities are determined by the limits of uniform atmospheric compression.

In the lung, therefore, each bronchial tube must act as an independent resonator, contributing to the resonant total a term of fixed pitch and quality. The resonance sum thus becomes a composite sound, in which the single sound terms form a consecutive series from the trachea or primary bronchus to the limit of sound production, with pitch ratios proportionate to those of the bronchial volumes, and the inferior pitch limit in the tracheal term. Under physical relations determining pitch in composite sounds there are clearly but two possibilities for definite pitch in normal percussion resonance as the sum of such a sonorous series. Now, in advancing pulmonary consolidation, which eliminates the small tube, and therefore

high-pitch resonances, the large tube, or low-pitch terms, must acquire a relative increase of intensity. Therefore assuming, as the first possibility, dominant intensity for the distal terms and consequent high-pitch normal resonance, that pitch must undergo progressive lowering as consolidation advances, with a limit at the point where tracheal resonance becomes the pitch term.

Again, assuming that the ratio is in favor of the tracheal term and normal resonance thus of low pitch, the same progressive consolidation can only affect the quality of resonance as determined by distal terms, and by purifying the sound make more prominent the pitch note, thus emphasizing its persistence. As no irregular ratios of intensity in the resonant series are supposable, it is plain that no condition can give percussion resonance with higher pitch than normal; and only with normal resonance of high pitch can resonance pitch be changed by pulmonary consolidation.

Although not strictly pertinent to a discussion of relations only, one form of intensity ratios in resonance terms is, perhaps, appropriately indicated here. It is evident that in this resonance series, starting with the trachea, each bronchial bifurcation adds two new terms. Thus, even if the tracheal term is strongly dominant at first, there may be a point at which the multiple additions will overpower this term some distance from the end of the series. The effect upon percussion resonance, however, would be simply one of degree. For if this ratio were actual, normal percussion resonance would be of high pitch, and undergo gradual lowering under consolidation to this suppositional point, and then take a sudden drop to tracheal pitch.

For some years I have used a definite pitch of percussion sounds, as determined by the tuning-fork, in both private work and teaching, but always with some doubt as to the accuracy of my ear. When, therefore, in the summer of '91 an opportunity was afforded to test my auditory acumen under the direction of Mr. Theodore

Thomas, it was not lost. The test was made on a patient with distinct dulness at the left apex, from tubercular infiltration. Thinking to make sure, first, of the identity of the sounds we were considering, I made percussion at the right apex over an essentially normal lung, at the same time asking Mr. Thomas if he appreciated a distinct resonant sound of definite pitch. Without the least hesitation he replied, in substance: "Certainly! That note is G—no, wait, it is not exactly G—it is a shade lower, but not quite down to F♯." Turning then to the left apex, I asked, just before percussing: "Now, how much higher is this sound than the other?" Almost with the first stroke came the question, in a tone of surprise: "What is that? How much higher?" Striking again I said yes, and repeated the question. The immediate reply was: "Not any. It is precisely the same pitch, and differs only in volume." Numerous repetitions removed all doubt as to the identity of the sounds heard, and suggested the explanation of the seeming paradox.

Some ten months later, in the spring of '92, it happened, by pure accident, that I used the same patient to illustrate to Mr. Walter Damrosch the nature of some questions I desired his assistance in answering. Again I percussed at the same right apex, asking the same question as of Mr. Thomas. Stepping to the piano and striking three or four notes, Mr. Damrosch replied: "Yes. The note is F♯—no! it is not exactly that, it is a trifle above F♯, but not quite up to G." I immediately struck the left apex, again asking: "How much higher is this than the other?" The answer was: "It is no higher. It is just the same." The former dulness, however, had largely disappeared.

I am happy to offer any gentleman who may so desire the privilege of testing the present resonance pitch in this same patient, any time before he leaves town; probably in early June or the last of May. Upon the basis of the above demonstrations I offer these propositions in summary of percussion:

1. Percussion product is a dual compound sound in which (*a*) percussion tone is developed by the thoracic bony wall, but modified by all contiguous solid tissues; and (*b*) percussion resonance is the sum of multiple resonances from the bronchial tubes.

2. Percussion tone alone is subject to variations of pitch; percussion resonance maintains a constant pitch, which is the same for all points of thoracic percussion.

3. Variations of percussion sound, commonly defined as normal or vesicular percussion, exaggerated resonance, dulness and flatness, are dependent very largely upon the *volume ratio* between percussion tone and percussion resonance, flatness being the entire absence of resonance. All variations caused by absolute change of pitch must come from percussion tone.

It affords me no little satisfaction to see how perfectly this duality of percussion sound harmonizes some apparently contradictory statements of eminent authorities regarding percussion pitch in emphysema, by showing that in this condition percussion tone may be high from the hardened costal wall, and percussion resonance apparently lowered from increase in volume.

Passing to the consideration of respiratory sounds, we find them classified as "vesicular, exaggerated, rude, bronchial, and tracheal" breathing. Composition and origin of these sounds are not definitely considered, but pitch relations are generally accepted as follows:

1. Inspiratory pitch of vesicular respiration is low and undergoes a progressive rise, through rude and bronchial breathing, to the maximum limit in tracheal inspiration.

2. The expiratory pitch of normal breathing is not universally agreed upon, but all authorities state that expiratory pitch, in all other forms of respiration, is higher than that of the corresponding inspiration.

3. Tracheal breathing is described as presenting, in normal, an extreme form of bronchial breathing and the constant pitch ratio of all pathological respirations.

The distinction between composite and compound

sounds, recognition of the multiple and independent origin of sonorous terms, and the restriction of exact and fixed significance to elements rather than sounds, are even more important in connection with auscultation than percussion.

With the admission that respiratory sounds are produced by the air current as affected by the bronchial anatomy, two conditions are necessary for such production: 1. A definite amount of atmospheric momentum. 2. Some condition causing sudden condensation or rarefaction of the moving air.

While these conditions may be present in a bronchial lumen, any resulting sound, except from transient localized constriction, must be of such subordinate intensity as to become a vanishing factor in the final product.

At the bronchial bifurcations, however, we find conditions most favorable for the production of sonorous vibrations, from which, with the clinical demonstration that, during respiration, such vibrations are developed in the trachea and primary bronchi, both inspiration and expiration are seen to be, of necessity, sonorous sums representing series of terms analogous to that of percussion, and maintaining similarly proportionate pitch ratios. The conditions controlling production of composite sounds from multiple terms, and determining their elements, or fixing limitations of value, previously defined under percussion, can be predicated directly of the composite sounds of inspiration and expiration. Identical questions, also, as to the possibilities of pitch modification from uniform pathological processes, are presented for determination.

It is not necessary, at present, to question the point, in bronchial bifurcations, at which production of respiratory terms ceases, or to determine definite ratios of initial intensity, since any variations in these ratios must be similar throughout the series. During inspiration each bifurcation not only diminishes, by deflection and interference, the intensity of every previous term, but contributes two

terms of higher and different pitch. Distinct pitch in a composite sound from such a series of terms clearly implies dominant intensity of end terms. For respiratory sounds, therefore, pitch becomes a question of a single large tube term with strong initial intensity as compared with multiple high-pitch terms of combined intensity. Whichever is assumed or proven clinically to control the pitch of inspiration, the demonstration, given under percussion resonance, is obviously applicable in this case, and the proof equally clear, that with high-pitch vesicular inspiration, derived from distal terms, progressive consolidation can only cause lowering of inspiratory pitch during the transition to rude, bronchial, or tracheal breathing; while a low-pitch vesicular inspiration, from dominant intensity of a proximal term, will not be affected by similar consolidation, although elimination of distal terms, as for resonance, purifies the tone and intensifies its pitch note. Hence all pathological forms of respiration caused by uniform pulmonary consolidation must present an inspiration of the same or a lower pitch than that of vesicular inspiration.

Tracheal breathing, as a distinct form of respiration, represents essentially elimination of all other terms, and thus becomes a single rather than composite sound, for inspiration at least. In view of the physical demonstration, and the innumerable and obtrusive musical illustrations, that pitch of sounds formed in tubes is in direct ratio to their size, it is difficult to understand how tracheal breathing ever came to be regarded as defining the superior respiratory pitch limits. Indeed, its position at the inferior pitch limit would seem to be a physical necessity, so obvious, from its relative calibre, as to compel recognition. Application of a stethoscope to one's trachea certainly affords a clinical demonstration which is conclusive. Upon the basis, then, of similar ratios of intensity between adjacent terms throughout the inspiratory series, it is proven that, whatever the pitch of

vesicular inspiration, rise of inspiratory pitch under uniform advancing consolidation is impossible.

The introduction of momentum, as an etiological factor in respiratory sounds, adds two contingencies, not associated with percussion, which must be indicated to include all the possibilities for general and uniform respiratory variations. Although the pitch of each respiratory term is fixed by the tube receiving the sonorous air, initial intensity of that term is dependent upon the atmospheric force (*vis viva*) and the amount of deflection as determined by the angle *of bronchial bifurcation.*[1] Reference to the anatomical relations of the bronchi shows that the effect of this motion and angle factor (m.a. for brevity) will not only be greater in the primary large tubes and low-pitch terms, since both m. and a. rapidly diminish centrifugally, but will be unequal in the different lobes. As a distinctly local condition it is foreign to the present paper, and further consideration is accordingly reserved for the discussion of definite values and special relations. I cannot pass the point, however, without noting the fact that in this relation we find an explanation of differences in respiratory pitch under normal conditions. It thus renders exact measurements of these differences by pitch and quality, and consequently earlier recognition of pathological changes at the apices, perfectly possible. As a second contingency: It is clearly possible that, under a low-pitch normal inspiration, with lobar consolidation sufficient to arrest, largely or completely, atmospheric oscillations in the primary tube of an upper or middle lobe, the air current of either normal or exaggerated compensatory inspiration, passing to the unaffected lobes, may develop a sonorous term, in crossing the mouth of the quiescent tube, of such intensity as to overpower the terms from the trachea or primary bronchi.

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[1] $\frac{1}{2}$ mv ^{2} sin. angle of deflection, as I remember it from college days, without vouching for its accuracy (if any one enjoys the pure science of this subject).

But the dynamic value of all respiratory terms is greatest in the direction of the respiratory current, so that, under the assumed conditions, inspiratory modification from this cause would be greater in the unaffected than in the diseased lobe, and the pitch raised above normal therefore only when there is complete cessation of air current in the primary tube. That condition, however, changes what was an open into a closed tube, with consequent drop of its pitch-note an octave or more. Therefore, since the earlier changes in this lobe, under gradually developing consolidation, would illustrate one or the other previously demonstrated possibility, it is evident that an inspiratory pitch, higher than in normal breathing, from this cause must be a condition of temporal as well as anatomical localization, and properly classed with local changes and compound sounds.

The mechanics of sound production is no less evident for expiration than inspiration. From statements given above it will be seen that the deflection of air current, which initiates sonorous vibrations at any given bifurcation, is, for inspiration, from the angular junction and adjacent walls of the two distal tubes, while in expiration these vibrations are developed by collision of the two air columns from the distal tubes as they enter the proximal tube. Thus, from every such sonorous centre, inspiration receives two terms, of different pitch, while expiration gains but one, and that of lower pitch than either corresponding inspiratory term. Therefore, since multiplication of these centres cannot modify this pitch ratio between inspiratory and expiratory terms, expiration of higher pitch than corresponding inspiration is impossible so long as every sonorous centre is represented in each respiratory sound. Relatively higher pitch expiration, in any form of breathing, thus necessitates: 1. Formation of the expiratory series by addition of distal terms to the inspiratory series; since elimination of proximal terms, by which the same effect might be produced, with low-pitch vesicular inspiration, is known not to occur. 2. That

inspiration shall derive its pitch from distal terms, that is, be of high pitch. But, with vesicular inspiration of high pitch, by previous demonstration, consolidation causes progressive lowering of inspiratory pitch. Hence low-pitch vesicular inspiration, rise of inspiratory pitch, with each modification, through rude and bronchial breathing, and relatively higher pitch expiration throughout the transition, being the pitch relations of respiratory sounds as given in all current works on diagnosis, *are, as associated conditions, physical impossibilities.* Again, with assumed high-pitch normal inspiration, expiration can acquire the requisite extra-distal terms only when the expiratory term from any given bifurcation is of greater intensity than the corresponding inspiratory terms. Omitting, for brevity, the steps of a somewhat lengthy demonstration, and risking its acceptance upon the accuracy of those already given, this conclusion may be stated: Although the absence of definite values for any of the factors of intensity, and the presence of some with fluctuating values, makes it impossible to give definite ratios of intensity, these factors, nevertheless, present such distinct relations as to render it a practical certainty that, under the conditions assumed, this ratio of intensity is in favor of the inspiratory terms.

That, clinically, expiration is frequently of greater intensity than inspiration, does not prove, as is easily assumed on first thought, the error of this conclusion, any more than the presence, in such case, of some condition modifying intensity beyond those assumed. With the above conclusion accepted, the final contingency disappears and leaves the demonstration complete, that relatively higher pitch expiration, as the result of uniform consolidation, is impossible except as an accidental condition.

Present statements, it is distinctly understood, relate solely to respiratory sounds, as developed under physiological conditions, or from pathological processes causing only uniform pulmonary consolidation. Conditions

associated with cavities of new formation; dual inspiratory and expiratory compound sounds; shifting bronchial contents; and isolated, accidental changes from any cause, are excluded, not arbitrarily, but in accordance with a classification of the subject required to permit accurate discussion, and are reserved for future consideration.

With this understanding I offer the following propositions, as defining the position I am prepared to defend:

1. Tracheal breathing presents a definite and the only fixed standard available for measuring pitch in respiratory sounds.

2. It presents, in normal, the inferior pitch limit of respiratory sounds and the universal pitch ratio, for uniform respiratory variations, in an expiration of *lower* pitch than inspiration.

3. The superior limit of respiratory pitch is not constant. It is found variously in vesicular, beginning rude or exaggerated respiration.

4. Respiratory variations between these limits are rarely uniform, and undergo only lowering of pitch, while maintaining the universal pitch relation between the respiratory sounds.

5. The pitch note of any respiratory sound, when compared with the corresponding tracheal note, becomes indicative of the point at which the dominant term is developed, and, after determination of the volumetric ratios of the bronchial tubes, may be an exact measure of pulmonary consolidation.

6. Respiratory sounds with relatively higher pitch expiration are exceedingly rare, clinically (I have yet to see a well-marked case), and are the result of accidental causes. Therefore they cannot be predicated of any specific pathological condition.

Although not pertinent to the present discussion, the essential absence from this paper of anything suggesting practical results in clinical work, is sufficient excuse for indicating recognition of the fact that in all matters

medical the Q. E. D. which closes demonstration entails a clinical "cui bono?" and for offering this reply:

1. Whatever physical statements are made should be accurate. If current statements are in error they should be corrected or omitted.

2. Physical relations afford the only fixed and certain factor in physical diagnosis, and, therefore, cannot honestly be disregarded.

3. It is perfectly possible and practicable to make use of these relations in clinical work, and to know thoracic conditions so far as they are indicated by physical properties of the affected tissues. Objections upon the score of difficulty are unworthy scientific medicine, and impossible from the standpoint of experience; while imputations as to the probable accuracy of results are admissions of conscious weakness, for, of the two factors involved, physics and physician, the former cannot err.

4. As applied to instruction, the analytical method is productive of larger, more uniform, speedy, and accurate results than are otherwise possible; and, what is of far greater importance to students of medicine, it affords a practical and reliable guide for personal, unaided study. This statement, evidently, must be offered and valued solely upon personal authority. It is based upon results obtained in private instruction during the past five years.

The foregoing demonstrations have been given only *in confirmation of facts previously determined by clinical investigation*, in order that acceptance of propositions, which present in definite form recognized facts, should not be requested upon personal authority. Their presentation as questions of pure science, with the promise of subsequent consideration of all contingent relations, justifies the request that they be so considered in criticism, and destroys every relation to claims of personal credit for their clinical determination or philosophical investigation.

In conclusion, then, permit me to suggest that in their discussion the motto should be, *Nil nisi scientia.*

DISCUSSION.

DR. S. S. BURT.—I wish to thank Dr. Quimby for his very interesting thesis, and also for his courtesy in furnishing me with a synopsis of the points he intended to make; for it helps me to grasp his meaning better than I might have done by simply listening to the reading of the paper, and it will also possibly keep me to the subject in hand, which is always a desirable thing in a meeting of this kind.

My experience in teaching physical diagnosis leads me to think that there is too great a tendency toward refinement of distinction. This is simply confusing to the students. The majority of them are not equal to such extreme differentiation. They do not learn as much, in my opinion, as they do from simpler explanations and demonstrations. There is an inclination among all of us in special departments to be carried away by the seeming importance of our own subject, and thereby to enter into many complicated and often unnecessary distinctions. We likewise allow too much latitude to the imagination. I find that I have to guard against these things myself, and, furthermore, think that I observe them, now and then, in other persons. As I understand it, percussion develops tone and at the same time resonance. Tone, which means sound, is the result of vibrations and is altogether a question of pitch, for a tone or a semitone, higher or lower, depends on a certain number of vibrations in a given time, which constitute pitch. Many vibrations in a second mean high pitch, and few vibrations in the same period mean low pitch. We go up the scale from C to C by tones and semitones, and we go down in the same way, and these changes in tone are changes in pitch. When we go up a tone—for instance, on a piano—it is due, as I understand it, to the more rapid vibration of a shorter wire, and when we go down a tone it is caused by the slower vibration of a longer wire. Modifications of tone being the result of variations of tension, any change in the density of the chest wall would of course affect the pitch, as the speaker says, and likewise any change in the solid tissues that are in immediate contact with the chest wall. Therefore an increase in the density of the chest wall or of the underlying pulmonary tissue must, by virtue of its increased tension, elevate the pitch. Now, resonance is the re-sounding of tone, and is hence nothing

but the prolongation of sound by reflection from cavities or by-vibrations in other bodies. We obtain resonance from either a solid body or from a cavity. We get it from the hollow intestines when we strike the wall of the abdomen, and from the almost solid liver substance. The resonance of the liver—for it is a mistake to say this flat sound is an absence of resonance—is a very different thing from that of the lungs or of the intestines. Nevertheless these various objects give forth both tone and resonance, and, what is more, it seems to me, resonance depends upon tone, and the two are of a necessity inseparable. These two things are not, as the speaker maintains they are, of independent origin and cannot be divorced from each other. In percussing the chest we develop at the same stroke both tone and resonance. If the tone is raised by the solid tissue the resonance must undergo a corresponding change, because it is the resounding of this raised tone and nothing else. I have always considered that dulness of the lung is a relative condition, and that as long as any healthy lung remains the dulness is to be considered a modified pulmonary resonance. But flatness and dulness, in my mind, are two different kinds of sound, for the former contains no element of pulmonary resonance. In these remarks I am trying rather to express my views of resonance than to prove that the speaker is upon the wrong track. If we chance to differ it is for the audience to decide between us. Such a paper as Dr. Quimby has given us to-night certainly requires more time than we have just now for a proper digestion. Pitch rises with dulness. I learned this and have always taught it, and I believe that Austin Flint would turn over in his grave if he were to hear that it does not. Pitch rises in proportion to dulness. In flatness pitch is always high, though it may differ slightly, no doubt, with different degrees of density. When we percuss the normal lung the pitch varies with the tension of the parietes, but the causes of the variation of tension need not now be enumerated. The speaker states—I give his own words—"Conditions associated with pathological cavities in the lungs are excepted from these (his) statements." Now, I cannot see why Dr. Quimby, in regard to the laws of sound, should make an exception of pathological cavities. It seems to me that the laws of sound must apply equally to normal and to abnormal cavities. There is certainly something illogical in such an

argument. With regard to the facts about consolidation, so far as my observation goes, let me state an experience which I have had quite recently. Since I received notice of this meeting Dr. Charles Knight sent a patient to me for examination. Dr. Knight, as most of you doubtless know, is not only an accomplished physician, but also an accomplished musician. He is a musician by inheritance as well as by education, and has been for the past twenty years a member of the Mendelssohn Glee Club under the direction of Mosenthal. By virtue of this he should have an acute ear for differences in pitch. He sent a woman to me who had tubercular laryngitis. He did not tell me that she had tubercular laryngitis, but sent her to me to get my opinion of the condition of her chest. I percussed her thorax, and, as sometimes happens in the flexible chests of women, it seemed doubtful at first whether much dulness could be demonstrated; but by placing her back against a door, to exaggerate the sound, I found dulness upon the right side in front and behind at the upper part of the lung. Not a great deal of dulness, but nevertheless dulness, and, as it seemed to me, accompanied by an elevation of pitch. Dr. Knight asked me to write him what I found. My answer was, moderate dulness at the upper part of the right lung, with high-pitched tone and resonance. Upon auscultation I heard broncho-vesicular respiration, which merged into bronchial breathing toward the upper part of the chest behind, where it was most distinct. That is, presumably, in my imagination I saw a tubercular deposit shading off into the normal lung. It was not a question, however, of what ought to be, but of what was, heard. The bronchial respiration was much higher in pitch in expiration than in inspiration. This I wrote to Dr. Knight, and he told me later that he found tubercular ulceration of the larynx, and he agreed with me that there was moderate dulness at the right apex, with a rise in pitch, and that there was bronchial breathing, with higher-pitched expiration than inspiration. It was a case of tuberculosis with laryngeal phthisis. I am not quite sure that Dr. Quimby maintains that there is no rise of pitch on percussion over a solid organ like the liver. The liver vibrates just as the lungs do, only in a somewhat different degree, because it is almost a solid body. If Dr. Quimby does maintain the point in question, my argument against it is in the form of similar testimony to his own.

In the presence of Mr. Frank Damrosch I percussed a healthy man over the liver, then over the lungs, and I asked Damrosch if he noticed a difference in the pitch. He said it was decidedly higher over the liver. I have been using my own ears in a musical as well as a medical way for some years, and have been also under the able instruction of Mr. Frank Damrosch in the Choral Club, but I shall not undertake to multiply evidence. Of course the amount is not so important as the quality of the testimony. I seriously think Dr. Quimby would have done well—I do not mean to make the slightest insinuation, for I have the greatest respect for Dr. Quimby's ear, and for his ability to distinguish those very fine differences of sound—yet I think it would have been a good thing it he had had some expert diagnostician confirm his observations. I do not question Dr. Quimby's ability to detect dulness; I simply remark that somebody might raise such an objection; consequently, in so important a point, why did he not get Dr. Delafield or Dr. Janeway or Dr. Loomis, or even Dr. Burt, to corroborate his diagnosis before he went to Theodore Thomas? There is sometimes a difference of opinion as to whether there is dulness in a man's chest or not, and I must confess that I am not wholly free from these moments of uncertainty.

With reference to respiratory sounds, Dr. Quimby says the normal respiratory murmur is of multiple origin and independent. It seems to me it is of multiple origin and somewhat interdependent—it is not wholly independent. Moreover, I would ask the question, how can vesicular breathing, or any sound that is audible, present no distinct pitch? Dr. Quimby states in the synopsis which he gave me that there are indefinite respiratory sounds that have no distinct pitch. The exact words are these: "Vesicular breathing is often of such extreme complexity as to present no distinct pitch"! I would like, as a matter of logic, to know how a sound can be heard that has no pitch? That which is audible, and yet without pitch, is something quite beyond my feeble comprehension. I cannot entirely agree with Dr. Quimby's statement that "in bronchial breathing the inspiratory sound undergoes a commensurate lowering of pitch, and expiration remains constantly, though not to the same degree, lower than inspiration." I have lately made a few experiments which seem to substantiate my opinion, though I have

not had time to go into the subject very thoroughly. Listening to a man's trachea in my clinic the other day, I found that the inspiratory was decidedly higher than the expiratory pitch, which was quite contrary to what I had expected. I asked a young man standing by me, who had a trained musical ear and was also a doctor, to listen, and he noticed the same thing. I then requested the patient to breathe deeply, and I found the expiratory murmur *higher* than the inspiratory. Here was a change of force and therefore a change of pitch, so that the *expiratory* became higher than the inspiratory sound. I then listened to a man who had bronchitis and asthma, and there I found inspiration higher than expiration over the trachea. My belief is that the pitch is variable; it is not always low pitch and not always high on inspiration. I shall try to explain why I think so. In a man with asthma the expiratory power is diminished; the elasticity of the lung is interfered with, and he does not expire with normal force. Therefore with every breath this patient took the inspiratory pitch was higher than the expiratory, which would bear out Dr. Quimby's idea; but in the other man the high pitch changed from inspiration to expiration, expiration being higher when he breathed with more force. My solution is that pitch depends upon the force of breathing. If the stress is on the side of inspiration, the higher pitch will be found here; if on the side of expiration, the higher pitch will be found there, and it is not, in my opinion, of a great deal of consequence which side it is on. In pneumonia I have noticed cases where the pitch was certainly low, and where it required all a man's power to determine the quality of the sound, but I ascribed this to the fact that the patient was breathing with diminished intensity. The consolidation was there, but air was not driven through it with enough force to give high pitch. I repeatedly have had to make patients breathe deeply in order to bring out the sounds; the air in the chest must be stirred up in order to develop the sounds. A parallel case is seen in valvular disease of the heart. There may be an obstruction or other lesion in the valve, but unless there is heart action there will be no murmur. When the heart is not driving the blood against the obstruction with sufficient force, the murmur will be indistinct. Nevertheless the lesion is still there. We must have life and motion to produce a murmur. So in the lungs there must be action to develop pitch, and

it may be sometimes higher in one place and sometimes higher in another, according to circumstances.

Dr. A. L. Loomis.—Dr. Quimby has certainly given us a very classical paper upon the applied physics of physical diagnosis. Some four or five years before the death of the elder Flint, he invited me to his house one evening to talk over the terminology of the physical signs which we were constantly using in our teaching, in order, as he stated, to arrive, if possible, at greater uniformity in the use of terms. I gladly accepted the invitation, and we had quite a long talk. He had in his mind, I suppose, at the time the preparation of a paper which he afterward read at the British Association. We did not differ in the appreciation of certain sounds heard in the chest in disease and in health, and, although we used different terms to indicate these sounds which we heard, we did not differ in our appreciation of the conditions which they indicated. I was impressed by a statement made by him at the time, that he did not believe it was possible for us ever to reach the same standard in the physics of physical signs which could be reached in music; that the laws governing musical sounds could not be applied to the sounds of the chest in disease or in health. In a conversation which I recently had with Mr. Frank Damrosch with regard to it, he made a similar statement, that he was quite sure that the laws of the musical note could not be applied to the sounds which we elicited either in percussion or in auscultating the chest. We therefore must recognize the fact, I think, that every man is to a certain extent a law unto himself in this matter. It would be delightful if we could apply the laws of physics as absolutely as Dr. Quimby has indicated this evening. To him it seems perfectly clear and his demonstrations perfectly satisfactory; but with me, my education has been from another standpoint. I did not commence studying physical diagnosis under the impression that all the sounds which I heard and all the variations of sound could be governed absolutely by the laws of physics. The first direction which I have always given students is to make themselves perfectly familiar with the normal chest sounds, both in percussion and auscultation; and until that has been done I think it is perfect folly to attempt to study sounds in disease. Those who make themselves most familiar with the normal chest sounds appreciate most readily the sounds of disease and make the best

diagnosticians. In commencing the practice of auscultation, listen over the trachea; let your ear become perfectly familiar with tracheal respiration. Then listen over the interclavicular space and let your ear become perfectly familiar with vesicular breathing. I do not attempt to explain to them. I do not believe it is possible to explain just how the normal vesicular sound is produced. There have been many different opinions expressed about it, and I do not think it a question of very great importance to the student. If he becomes so familiar with these sounds that he will recognize them whenever and wherever he hears them, he has mastered the first principle of auscultation. If you hear a violin in another room you know that you are listening to a violin, because you have heard the instrument before. So in examining the chest, one becomes familiar with the normal sounds, and then appreciates any deviations from the normal standard. He says, perhaps, "I am hearing tracheal breathing in the infraclavicular space." Perhaps not. What indicates consolidation of the upper portion of the lung? It is not necessary to enter into the elements of sound in order to appreciate this. You simply appreciate that tracheal respiration is heard in an abnormal situation, and so you go on appreciating all the deviations from the normal standard. Afterward you study these deviations in their relation to the different elements of sound.

In percussion there is undoubtedly a resonance of the chest wall, a sound which you produce by percussion on the chest wall, and that sound is modified by the condition of the tissues underneath. If you are familiar with the mechanical principles of percussion, and you percuss and do not get the normal sound, but get, for instance, dulness where in the normal condition you should have resonance, you know that there is consolidation under the point of percussion. The greater the consolidation the more complete the dulness. If the lung contains more than the normal amount of air, as in pulmonary emphysema, a very different sound is elicited. I cannot describe emphysematous percussion and give its exact elements, but one who has heard it, has heard it until he knows it, will detect it as soon as he strikes an emphysematous chest.

Now, it seems to me that any man who attempts to learn physical diagnosis without practice, without going

over the ground again and again and making himself perfectly familiar first with the normal sounds, and afterward with the diseased sounds in all varying conditions which disease produces in the chest, will never make a good physical diagnostician. I am not saying anything at all against Dr. Quimby's methods. The way in which he has managed the subject is certainly very creditable, and he has perhaps given a higher standard than I, but he has commenced entirely from a different standpoint. Whether he can make a better diagnostician by his method or not is a question.

It seems to me that terms used for a long time come to express certain things to the one who has become accustomed to them. In teaching I employ certain terms to indicate certain diseased conditions. You might ask me how I know they do. I know it partly by clinical observation and partly by post-mortem demonstrations. In the dead-house one finds out his mistakes, and by correcting these mistakes he finally comes to be pretty accurate. If a patient comes to me and says a certain man whom I know to be a thorough diagnostician finds nothing abnormal in his chest, and I find evidence of disease, I feel certain that either the physician has not made a careful examination or that there has been a great change in the sounds since he saw the patient. For there are certain conditions which every man familiar with physical diagnosis will appreciate and will recognize as indicating diseased conditions.

One other thing. We have talked a good deal about vesicular respiration, and we know how it is produced. Dr. Leaming used to take the ground, when we were both connected with the Demilt Dispensary, that he could hear vesicular respiration while the patient held his breath, which he called supplementary vesicular breathing. Absence of the supplementary vesicular breathing indicated to him changes in the pleura. If he did not get this vesicular sound after the respiration had stopped, he claimed there were changes in the pleura which prevented it. We all know how he believed that the pleura is the starting point of disease in the lungs in most cases of phthisis. Now, I was never able to hear that. Supplementary vesicular breathing I have listened for time and again, with Dr. Leaming standing over me, and I never could hear this sound after the patient stopped breathing. He must

have heard it. It is difficult to explain these auditory differences.

DR. C. E. QUIMBY said: In reply to Dr. Burt's remarks, so far as they relate to the points at issue, I think it sufficient to refer him to those portions of my paper in which the questions raised are considered. He will find there reference to authority for physical statements and demonstration of my claims. I must again request recognition of the fact that this paper deals solely with questions in acoustics, and that personal opinions or views are not pertinent to a scientific discussion. Nevertheless, since silence may imply acceptance of criticisms, I am forced to indicate some errors. This one statement will sufficiently illustrate all: "Tone, which means sound, is the result of vibrations, and is altogether a question of pitch, for a tone or a semitone higher or lower depends on a certain number of vibrations in a given time."

It is difficult to imagine a more complete illustration and proof of the pertinence of the preface to my paper. In the first use of the term "tone" it evidently must have its broadest significance, with which it does mean *sound*, but only because it means *a sound* of definite pitch and characteristic quality. It *is* unquestionably the result of vibrations, and the *truth* of that statement is not affected by the fact that those vibrations *must be multiple sets* of vibrations. The next clause offers an uniquely beautiful illustration of deceptive language. If we are to find the unexpressed subject of "*is*" in the first word "*tone*," the statement becomes totally erroneous, for quality is a more prominent thought in that tone than even pitch. But, although such interpretation would be most natural, since the "and" preceding this clause necessarily connects backward, the "*for*" introducing the following clause as necessarily connects forward, where, by simple repetition of the term "*tone*," apparent continuity of meaning is maintained and suspicion arrested, although there has been an instantaneous transition to the purely technical meaning of the term, as used to indicate musical intervals, which are simply mumerical ratios of vibration: $\frac{8}{9}$ and $\frac{9}{10}$ being the two tone intervals, differing by what is called a "*comma*," and $\frac{15}{16}$ being the semitone interval. With this "*tone*" as the subject of "*is*," the remainder of the sentence becomes absolutely true. It hardly seems necessary to suggest that conclusions based upon such postulates cannot be entirely free from uncertainty.

In the first preparation of this paper all these acoustic terms were considered. In final revision that portion was omitted, because I was ashamed to assume its necessity in a paper addressed to scientific men. Nor will I now admit that anything more than indifference or carelessness causes it to appear desirable. I will say, however, that I shall not always submit in silence to having my work labelled "finical refinements," or being designated myself as "*pedant*" for no greater cause than a moderate familiarity with the elemental definitions and principles of acoustics. I am fully aware that universal consent and convenience have justified the use of terms with a meaning that strictly is absolutely false. Even the statements regarding the cause of pitch, as given in authoritative works on acoustics, are not only untrue but are shown to be false. As a form of auditory sensation, which pitch strictly is, it *does not* depend upon the number of vibrations, per unit of time, of the sound-producing body, but upon the number of vibrations *striking our ear* during a time unit. Every one has experienced the demonstration of this statement, when travelling, in hearing the rising-pitch scream of an approaching locomotive whistle, and the instantaneous drop of pitch as the locomotive passes. But as this factor of motion as determining pitch is never present in practical acoustics, current statements as to pitch origin are *practically* true. This license of convenience is similarly illustrated in the use of the term "law" as applied to physics. It is a more convenient term than *formula*, and is not misleading so long as we remember that it means the universal "*is*," not "*should be*"—what we accept, not dictate.

In reply to Prof. Loomis, I can only express extreme obligation for the position he has assumed toward my paper. My greatest difficulty in private discussion has been to convince gentlemen that the methods presented are not only *not antagonistic* but perfectly adjuvant to those in use.

I am well aware that this has been due largely to the fact that a scientific demonstration of my own position necessitated contradiction of many established and unquestioned beliefs. For only by proving the complete absence of physics from current methods was it possible to reveal their value as mechanical methods pure and simple, and so gain recognition of pure physical analysis as affording a distinct method of diagnosis by itself. To

have this claim so completely accepted, therefore, and the absolute independence of the two methods not only admitted but comprehensively defined, as Prof. Loomis has so conclusively done by his succinct exposition of existing methods and the specific statement that "it is not necessary to enter into the elements of sound in order to appreciate thoracic sounds," was most unexpected good fortune and leaves nothing more to be desired on that score.

His indication, also, of the absolute opposite standpoint from which the physical method approaches the subject renders impossible any suspicion of antagonism, and establishes my method of pure science in a position justifying a demand for its scientific investigation as a possible source of good and an impossible source of harm.

No one could have enjoyed the personal acquaintance with Dr. Loomis which has been my good fortune for nearly ten years, and hesitate for an instant to give unqualified assent to any statements as to what is already possible in the direction of diagnostic acumen. Yet upon one point I shall claim knowledge vastly superior to his. Dr. Loomis, if I mistake not, went directly from his hospital service in Bellevue to a dispensary class, and almost as quickly returned to the hospital as visiting physician. Both the ante- and post-mortem sources of instruction have always been at his command in unlimited quantity. Within forty-eight hours after leaving the same hospital service I was in New England, and very soon watching my shingle bait for daily bread, in a place where dispensaries and post-mortems are hardly more real than hobgoblins and fairies.

Neither Dr. Loomis, nor any one else devoid of the experience, can ever know what a wealth of sarcasm such surroundings can develop in his words, "you must make yourselves perfectly familiar with the normal chest sounds and afterward with those indicating disease," when, from long abstinence, the very thought of a patient excites the salivary glands to almost painful activity.

However slight the value of pure physics where clinical advantages are unlimited, the analytical method of physical diagnosis cannot be without interest for the mass of the profession, so long as men must enter practice prepared to make physical diagnoses only by the limited clinical experience at present afforded. I trust I shall not seem to exceed the limits of modesty, however,

if I reaffirm my claims as to the value of this method, made after some five years' experience, when so many gentlemen have not hesitated, in private discussion, to deny absolutely my statements upon this point without even knowledge of the method, much less experience in its application.

Stated Meeting, March 2d, 1893.

CLINICAL NOTES ON THE DIAGNOSIS AND TREATMENT OF PLEURISY.

By BEVERLEY ROBINSON, M.D.,

NEW YORK.

For many years past I have felt convinced that a discussion by physicians of experience in regard to several points in the clinical history of pleurisy would be instructive.

To some general practitioners, I know, the diagnosis and treatment of pleurisy seem one of the most ordinary subjects of medicine. Indeed, I have heard one rather distinguished member of our profession affirm that, so far as pleurisy with effusion was concerned, he could invariably recognize it with certainty, and that the physical signs are always clear and distinct.

Such, I am free to say, is not my own belief. True it is that when the effusion is very large the signs are usually so well marked that any one accustomed to see and treat many such patients will rarely be deceived. In the majority of such instances the flatness over an extensive area of the chest posteriorly, in the axillary region, and up to the second or third intercostal space in front, combined with entire loss of respiratory sounds over the whole region involved, will unquestionably, where the symptoms of the case are corroborative, enable us to be absolutely affirmative in regard to the nature of the disease. On the other hand, whenever the effusion is moderate we shall at times be in considerable doubt as to the presence of this fluid in the thoracic cavity, unless we have recourse to one or more punctures of the chest wall with an exploratory trocar of a proper size.

This doubt may depend upon the variety of physical conditions incident to pleurisy with moderate effusion. In the first place, the chest walls may be very thick, owing to heavy masses of muscular tissue or of subcutaneous fat. This condition frequently renders absolute certainty in the interpretation of percussion sounds well-nigh impossible. We readily obtain dulness, or even flatness, but whether or not the dulness be due to the conditions just mentioned or to the enclosed pleuritic fluid we are at a loss to determine accurately. Of course the dulness or flatness may be due to a combination of the two conditions; but how determine by percussion alone whether there be the single condition or the two conditions conjoined?

Under these circumstances, we naturally and properly recur to other physical signs and auscultate the respiratory sounds. Frequently, in these instances, both I and my resident house staff at St. Luke's, Charity, and elsewhere have found the respiratory vesicular breathing heard to the base of the lung, perhaps slightly modified at times as to its intensity and characteristics in a notable manner and degree, perhaps heard apparently in such a normal manner that we were at a loss precisely how to interpret what we found.

In some persons of stout or bulky frame the breath sounds are heard feebly across the thick chest walls. It may be that this condition explains the fact. Again, however, some patients breathe poorly. They do not expand their chest walls with each effort of inspiration when it is carried on in an ordinary manner; and even when we urge upon them to dilate their chest or breathe deeper, it seems as if the breath sounds come to our ears feebly; this especially being true in regard to the breath sounds as heard at the base of the lungs. Occasionally we are suspicious in such cases that there may be some old pleuritic adhesions which obscure our hearing. And not only is this our suspicion when facts in the preceding history of the patient appear to render this suspicion

probable, but even in those cases where we have only a conjecture as to what may have occurred. For example, we do undoubtedly find, post mortem, in numerous cases and of all kinds, old pleuritic adhesions more or less extensive.

Leaving out those cases where chronic lung changes of tubercular or fibroid nature appear to be connected with the pleuritic thickening, there are some which I am convinced are connected with badly determined previous conditions which have been present during life, and which have never been noted or well understood. I am inclined now to believe that some of these conditions were of the nature of obscure forms of rheumatic or gouty dyscrasia, affecting the pleura in an insidious manner, just as they do fibrous or other serous tissues of the economy. Such pleuritic adhesions or thickening may not be in any sense a sudden growth or development; they may come on gradually, without occasioning any very manifest symptoms either to physician or patient, and yet they may be none the less a reality, and in just such cases as I have referred to they may be actually present and obscure an otherwise clear diagnosis.

In some instances the breath sounds may be heard to the base of the lung posteriorly, and their intensity be but slightly diminished. With these auscultatory signs we may find more or less numerous moist râles, superficial in character, rather fine or coarse in quality, and heard in one or both times of the respiration. What is the import or significance of such râles? At times we cannot understand how they should exist if a moderate effusion is present; and particularly is this true if our faith in the descriptions of pleurisy as written in our classical text books be our implicit guide as to what our diagnosis should be. Yet that such râles are often found when two or more pints of serous fluid fill the pleural cavity, I have more than once been able to demonstrate in the most convincing manner by withdrawing such an amount of fluid by means of the aspirator.

These râles were stated by the late Dr. Leaming to be of the nature of pleuritic râles, and many of us remember with how much talent and ingenuity he endeavored to prove the correctness of his assumption. I have never been convinced that Dr. Leaming's ideas were correct, at least in anything like the general manner he wished so earnestly to have us accept them. The very cogent reasons which made me doubt Dr. Leaming's affirmation were, first, because it seems to me that the two layers of the pleura must usually be kept so far asunder by the amount of effusion present that it was almost impossible to believe that there could be any friction between adjacent pleural surfaces and thus produce the pleural râles or friction sounds. Of course I am free to admit that not infrequently there are probably some fibrous septa between the adjacent layers of the pleura at certain points, which do not interfere notably with the accumulation of a tolerably abundant pleuritic effusion, and yet occasion adventitious sounds in the pleura, which are most confusing when the question of accurate physical diagnosis is under consideration. I am, however, disposed to believe in more numerous cases that the true explanation of these so-called pleuritic râles of Leaming lies in the fact of the congestive condition of the pulmonary structure underlying the visceral pleura. During normal respiration, in many chests, the superficial layers of the congested lung are sufficiently expanded to produce a comparatively large number, more or less widely disseminated, of moist râles of different sizes, usually carried to the ear, as if they were superficial rather than deep-seated. We must admit, however, that such sounds are frequently confusing, and we shall be prone to acknowledge, unless repeated experience has proven to us how deceptive they are, that they are incompatible with the presence of a moderately abundant pleuritic effusion.

Those instances in which I have made more than one mistake of diagnosis in an evident manner are the cases of pneumonia met with intercurrently with a severe at-

tack of *grippe*. In some of these there were markedly diminished breath sounds, dulness over an extensive area of the lung posteriorly, and disseminated fine moist râles at times—no bronchial breathing; diminished vocal fremitus and thoracic vibrations; slight bronchial expectoration, and that, too, of a negative character. I have repeatedly tapped such cases and found not a drop of pleuritic fluid. I have afterward been able sometimes to be convinced as to the precise nature of the morbid lesion I had to deal with, without causing my patient the slight but useless distress of a puncture or punctures, which would result negatively.

In pleurisy with a notable degree of effusion we usually expect both vocal fremitus and thoracic vibrations to be diminished very considerably; and yet such is not invariably the case. I see cases quite frequently in which neither of these phenomena are present, and, on the contrary, the vocal sounds as well as the vocal fremitus are, so far as I could determine, almost normal in quality, or *timbre*, as well as intensity. How shall we explain such facts, so much opposed to the prevailing belief and to the descriptions as given by many classical authorities? I presume that it must frequently depend upon the unequal amount of tension in different cases, even with an amount of fluid contained in the pleural cavity approximately similar. The tension of the chest walls may depend upon the less or greater mobility of the bony framework of the thorax, the greater or less power and action of the exterior muscles, the possibly greater or less capacity of the pleural cavity itself. In other cases, and notably where the pleural effusion contains an increased amount of fibrin, or where pus cells are more or less abundant, the density and nature of the intrapleural fluid will no doubt influence materially the signs to which I have referred.

I would add, in addition to what I have already said, that the modification of the breath sounds, either in regard to the more or less well-marked change which we term

"œgophony," or the soft blowing sound so characteristic at times of compressed lung, caused by effusion in the pleura, rather than of the solidified condition which is found in pneumonic infiltration, is variable in nature and amount.

In regard to the nature of the effusion in pleurisy, I would direct attention to the relatively great infrequency of blood in notable quantity contained in what are proven to be, later on, cases of acute pleuritis without underlying physical conditions which manifestly account for it, such as tuberculosis or carcinoma. Still, among a large number of cases very closely observed, I can recall distinctly one instance in which the sanguinolent nature of the effusion seemingly had nothing to do with any constitutional condition which became evident at the time the effusion was present or subsequently. In this case the patient recovered completely after two tappings and has never had a return of his disease, although many years have elapsed since I took care of him. Of course, as is familiarly known, effusion of serum mingled largely with blood means, within the pleura, very often tubercular or carcinomatous infiltration. In the one as in the other disease a long time may elapse before the diagnosis may become assured; but by careful watching and research the correct diagnosis can usually be made.

Of course it is a relatively simple matter, when the diagnosis is clearly established, to point out the probable prognosis and lay out a rational treatment of such cases. I desire earnestly to urge upon my listeners, in these doubtful cases, to have frequent microscopic examinations made of both sputa (if there be any) and chest effusion.

Only a few weeks ago a young man left my care in a private room at St. Luke's to go to the sand hills of Georgia, because we found definitely, and although in some particulars his condition seemed much improved, unquestionable evidence in his sputa of the probable tubercular nature of a double pleurisy from which he

had been suffering for a long time. When this young fellow came under my professional charge he had on one side a relatively pure serous effusion, moderate in amount; but on the other the effusion contained a considerable quantity of blood. Although for many weeks, and despite the most careful, repeated research, no distinct proof could be found of tubercular disease, at the end of this time it came to light. In this case, as in others formerly observed, the nutrition, temperature, and general condition of the patient had notably improved under judicious care and management. When, however, tubercle bacilli were discovered, I concluded—and I believe properly—that the hospital was no longer a suitable place for such a one, and urged strongly the departure of the patient for the South, where sunshine and temperate air could be more largely enjoyed during our winter and early spring months. The result attained has apparently answered all my just expectations, for within a brief period my patient is like a new being, and I am convinced that with good surroundings and time his tubercular condition will become arrested or non-active.

Localized pain in the side as a symptom of pleurisy, especially in its first stage and before the effusion has become evident, is a familiar one to all practitioners of medicine. Usually this localized pain in the axillary region, or elsewhere in the chest, is accompanied by friction sounds which are easily detected by auscultation. There are numerous instances, however, in which the localized pain is indeed the only local symptom which we can discover. This pain is not increased by pressure and not invariably by respiration or cough. Accompanying it there is frequently a dry, irritative, obstinate cough, unaccompanied by any rise of temperature. The throat symptoms are slight or negative, and, indeed, we are unable to find anything satisfactorily explanatory of the cough. Finally, after many weeks of useless medication and at the end of our resources, we begin to be fear-

ful lest there be an underlying tubercular condition which is the source of local irritation.

If in these instances there be any great relative difference, as is sometimes true, between the respiratory sounds, we are more anxious still in regard to the ultimate outcome of the case, and propose, it may be, a complete change of climate. I am convinced to-day that many such patients are suffering merely from a local inflammation or thickening of the pleura, and that this condition is not, as is frequently stated, the immediate result of "catching cold" or atmospheric changes, but rather a localized expression of a rheumatic or gouty dyscrasia, inherited or acquired.

Frequently there are other very evident rheumatic conditions which are present at the time of the irregular pleuritic attack, or there is a history of one or more previous attacks of acute or subacute articular rheumatism. In other instances there is little or nothing to guide one, except the frequent recurrence of these attacks, the absence of definite symptoms pointing to tuberculosis, and the results obtained by antirheumatic treatment. I have even now present to my mind a lady patient of mine who has been a source of considerable anxiety to myself and to all the members of her immediate family, on account of just such attacks as I have referred to; but fortunately, after very careful and long-continued observations, I have concluded that her recurrent attacks of localized pain and obstinate cough are due to a localized rheumatic pleurisy. In this patient I am also confident that overfatigue in household duties, which occasions a general depression of her vital forces, and particularly lessens the muscular energy of her cardiac contractions, is the efficient cause of the attacks of rheumatic pleurisy, rather than the so-called colds to which she attributes all her ailments. In this instance the attacks of pleurisy are not accompanied with effusion, as a rule, and when the effusion is present it is very slight in amount, at least so far as the closest physical investigation will enable me to

determine. It is sometimes extremely difficult to decide by any commemorative signs or physical exploration, other than the use of the aspirating needle, whether or not the serous effusion has become sero-purulent or purulent. In some instances the temperature curve is about the only probable diagnostic sign. Whenever we notice, after a week or more of the duration of a pleurisy with effusion, and when tubercular disease may be probably eliminated, that the temperature curve shows intermittent high elevations and equally low falls, at somewhat irregular intervals, accompanied or not by rigors and more or less profuse sweating, we must be on our guard against the presence of pus in the pleural cavity.

This statement is still more important if the pleurisy be a secondary one, or a complication of some acute febrile disorder, as typhoid fever, pneumonia, or the exanthemata. If the pleurisy be a complication of chronic cardiac or renal disease, suppuration occurs, but less frequently.

Localized œdema over the side of the chest affected with an empyema which has occurred in an acute or subacute manner is a frequent but not invariable sign. It may be slight or moderate in amount. It may not be present until the empyema has existed for many days. Of course, when it is present, and particularly if it be adjoined to recurrent chilly feelings and rapid rises and falls in the temperature curve, it means almost invariably, in the premises, a suppurative effusion in the pleural cavity. In all cases in which there is an obstinate dry cough, or one with slight frothy expectoration, in pleurisy, which we are inclined to attribute to this disease, we should not be satisfied without making a very thorough examination of the throat. A relaxed uvula, a subacute pharyngitis and laryngitis with more or less tracheal irritation, quite frequently are really the cause of the rebellious cough, and judicious topical applications will soon clear up the situation materially by curing a cough which already gave distress and anxiety.

Incidentally I may be permitted to remark that nothing

disturbs me more than to hear occasionally of the banishment of a patient to Colorado, the Adirondacks, or the sand hills of Georgia, for cough which seems to forebode incipient phthisis, when this symptom depends solely upon an undiscovered irritation proceeding from the upper air tract, and which could be readily and quickly relieved by local treatment.

In this connection I would also remark that in latter years I am of the opinion that there is too great a tendency to the belief that acute pleurisy means frequently an underlying tubercular cause, and that the pleurisy is merely a secondary effect of the tubercular deposit in the outer layer of the lung or within the pleura. Of course we see such cases, but they are met with habitually in hospitals and in persons who have already a depraved constitution and who have given evidences of a tubercular tendency. But to believe that in a previously healthy individual an attack of acute pleurisy will probably be followed by pulmonary phthisis in the near or distant future, is to go, in my judgment, very wide of the mark. In all published records of this kind it is well to know and bear in mind that hospital statistics mean one thing, and the statistics obtained from close observation of private patients quite another. I cannot bring this truth to your minds too forcibly, because it is far too often lost sight of and helps vitiate and render wholly inaccurate many reports of good observers, when these reports are translated to mean what occurs really in all cases of the kind referred to.

The question of the treatment of pleurisy with effusion is always of interest from many points of view. In the first place, and even when moderate in amount, it may and often does produce quite marked functional disturbances, some of which, as dyspnœa and cyanosis, may be of sufficient importance to inspire solicitude. These symptoms may appear earlier and be more pronounced in weak patients, or those already suffering from general anæmia or feebly acting hearts. Even when a moderate

effusion causes no special anxiety at the time on account of the functional disturbances of different internal organs occasioned by its presence, yet it is always a menace to life, in my judgment, because it may rapidly increase in quantity and become excessive. Of course the likelihood of this sudden increase, other things being equal, may depend upon one or more of several factors, such as constitutional tendency or individual condition at the time of the attack, surroundings, treatment, etc. If an effusion has lasted without appreciable diminution for more than two weeks, it is unlikely to disappear under ordinary medicinal methods of treatment without leaving in its wake a condition of more or less compressed lung and thickened pleura, which will only disappear little by little and in quite a long period of time, even under the best of circumstances. Moreover, the effusion itself may remain stationary so far as quantity is concerned, and besides is liable to change spontaneously from being a simple serous effusion to one containing numerous lymph flocculi or pus cells—in other words, into a sero-purulent effusion.

I have given a fair trial, during my hospital services in past years, to the different methods of medicinal treatment which have been in vogue temporarily, and for which was claimed by their too enthusiastic promoters more than a due amount of credit in making an effusion disappear rapidly. As a result of these trials, after carrying out other rational indications during a few days, if the effusion is moderate, not to say abundant, I believe that it is far wiser to withdraw the fluid by means of aspiration than to trust hopefully to the curative influence of uncertain medication. In those instances, indeed, in which any special medication has been apparently of great service, usually to the thoughtful physician it will be discovered that the true cause of the results obtained resides mainly in the fact that a dyscrasic condition, or organic change of a special and important organ, though it may be far removed, has not been ignored. Of course, if a patient who has pleurisy has been habitually a suf-

ferer from hepatic engorgement, repeated small doses of calomel, podophyllin, or ipecac produce a purgative and diuretic effect, which may diminish the pleural effusion in a very notable degree by allowing the absorbents in the pleural layers to take up its serous effusion in a rapid manner. In a similar manner, and as has been already noted, if the rheumatic or gouty condition be the efficient cause of the pleurisy in the first instance, as in all inflammatory states of like origin, whether it be in the joints or on the pericardium, the endocardium, or the fibrous insertions or sheaths of muscles or tendons, salicylate of soda and colchicum in some form or combination will be rapidly and unquestionably very effective as to their remedial action. Finally, if a weak heart renders passive congestion more or less imminent in every internal organ —lungs, liver, kidney, spleen—why should the pleura, when inflamed accidentally, not be wonderfully influenced by cardiac stimulation with strophanthus and digitalis? Some one of my hearers may be inclined in this place to criticise the writer and remark that these are but trite aphorisms with which all good practitioners are familiar and upon which they invariably base their conduct. It may be often true, and among those probably who may honor me by discussing this paper I have little doubt that this matter, as well as all other points upon which I have briefly touched, is to them, as it were, a "twice-told tale"; to many of my colleagues I must believe, judging by my frequent observations, they are truths more "honored in the breach than the observance." Whenever a notable amount of pleuritic effusion is present, I now hold that it is almost always good practice to remove it by aspiration, and that, too, within a very few days of its formation. True, it may and does form again once or twice, but the re-formation is of a less quantity of fluid, and one or two aspirations will very often get rid of an amount of effusion in a perfectly safe and almost painless manner that requires weeks to remove by the very best medicinal treatment with which we are at present fami-

liar. If aspiration did any real *harm* in any but the smallest percentage of cases, I could understand how it is that the medical profession still delay its use for several weeks, or until ordinary medicinal methods have been found ineffective. So far as my experience goes—and it is *now*, I believe, a large one—I have very rarely seen aspiration of a serous effusion do any harm that could be fairly attributed to it when it is performed with ordinary care and skill. Of course the aspirating needle employed should be rendered absolutely aseptic.

In addition, the skin over the region where the puncture is made must be carefully washed with bichloride solution 1 : 1000. The needle may be thrust directly into the pleural cavity, or a small incision of the skin may be made in advance. A hypodermic of cocaine, four-per-cent solution, is preferably employed before the puncture or incision in extremely nervous patients, as it abolishes the slight pain connected with the operation and to a certain degree quiets mental apprehension. The patient should be in a lateral inclining position, with the side to be operated on turned upward and well supported by a firm pillow. I give these minor details, because apparently they are occasionally lost sight of, despite their supposed familiarity to all. It is wise in specially weak individuals to administer an ounce of whiskey and three to five drops of tincture of strophanthus fifteen minutes before the operation. It is unwise to withdraw the entire quantity of a very large effusion at one tapping in any case. In moderate effusion the entire quantity may be safely withdrawn at one operation, provided the lung is not notably compressed and the effusion has not been present in the pleural cavity during many weeks. More than once, and even in very large effusions, I am of the opinion a withdrawal of a small quantity of fluid has been of very great value, in that it has given the absorbents a chance to take up the fluid rapidly, and this was clearly shown by its subsequent speedy disappearance. The pressure on the vessels, or the tension

of their walls from within, has, previously to the thoracentesis, prevented this useful absorption of fluid. My impression is that we are indebted originally to Dr. A. A. Smith, of this city, for the observation of the fact to which I have referred, and which clinically, I believe, it is important to recognize.

One of the great dangers to which a patient is exposed from whose chest a large quantity of effusion has been rapidly or at one operation removed, is unquestionably the sudden appearance of œdema of one or both lungs, combined with rapid congestion of the compressed lung. Such patients will sometimes, within an hour or more after the operation is terminated, begin expectorating an abundant amount of frothy, serous sputa, which are combined with a constant irritative cough. The latter symptoms often cause very great prostration, and sometimes death is thus brought about in a very sudden manner. Upon auscultation the lung bases, and sometimes the entire pulmonary structure on both sides posteriorly, are filled with a large number of moist râles indicative of the condition occasioning it. Dry cups to the back and alcoholic stimulation, together with frequently repeated doses of nitroglycerin and atropine, by the mouth or hypodermically, according to the urgency of the symptoms, is now, I believe, the admitted best manner of treating this accident.

Does aspiration increase the local inflammation at the seat of puncture? I do not believe it does, as there is no evidence in the general signs or local symptoms to justify this belief in the great majority of cases. Does the fluid reaccumulate in the pleural cavity when it is withdrawn too soon in acute cases? In many cases it does, but it is usually in diminished quantity, especially if the chest wall of the affected side be firmly strapped immediately after thoracentesis. Sometimes the fluid in effusions that have lasted only a week or two, when once removed, does not recur again, or if it does it is merely a return in very much diminished quantity which occasions no

appreciable disturbance of functions and will sooner or later completely disappear. This statement is not, I believe, in accord with much that is authoritatively written. Does a serous effusion frequently become sero-purulent or purulent after one or more operations of thoracentesis? Undoubtedly it does, but when it does I am inclined now to believe that it depends upon one of two factors: either the needle or skin is dirty or non-aseptic, or else there is a focus from which the pus originates under the dependence of some dyscrasic condition, as a typhoid ulcer, or from an unrecognized suppurative condition in or about the appendix, etc. Of course at times the suppuration may originate in the pleural cavity itself, and without our ability to discover any different locality as the starting point of the purulent process. It will be noted, therefore, that *I do not admit* at all, as is often done, the probability nowadays of a purulent change in a serous effusion caused by the thoracentesis. This lamentable occurrence no doubt frequently occurred in former years at a time when aseptic surgery was an ignored or unknown art, but this propagated view should, in my judgment, be relegated to the past and in no sense be the guide of our present operative treatment of pleurisy with effusion.

Much as I believe in the manifest advantages of thoracentesis in all effusions in which the fluid remains serous or only contains a small quantity of pus, I am decidedly of the opinion that when the effusion becomes markedly purulent—and this statement is practically true, with few exceptions, amongst adults—recourse should be had to excision of a rib on the affected side, the introduction of a drainage tube and proper antiseptic dressings at the time of, and subsequent to, the operation. In these cases all washing out of the pleural cavity should be avoided, unless the putridity of the purulent exudation or the intermittent high rises of temperature indicate clearly the necessity of such irrigation.

In concluding this desultory paper I would say that I

have requested Dr. Dade, the present resident physician of St. Luke's Hospital, to bring here with him what is known as Allen's pump, which we now use at St. Luke's Hospital for aspirating purposes in preference to the ordinary Dieulafoy's aspirator, mainly because it is simple in construction and withdraws the fluid more easily and rapidly than does the aspirator. I would add that the pump is relatively cheap and may be adapted to many different purposes, such as irrigation of the stomach, the bladder, etc. I would not mention this pump unless I thought it were an addition to our surgical armamentarium and not adopted as yet practically by the profession in general.

Since writing my paper Dr. Gibson, formerly house surgeon at St. Luke's Hospital, and who has lately returned from Vienna, has shown me a form of aspirator manufactured by Leiter which seems to me to have special advantages. These latter, with your kind permission, will be shown by Dr. Gibson after Dr. Dade has demonstrated the use of the Allen pump.

DR. GIBSON, at Dr. Robinson's request, showed Leiter's aspirator, and said:

This pump is made by the well-known instrument-maker of Vienna, Leiter, and I think it is perfectly reliable. The chief advantage is that it is so simple. There is no part but what can be made perfectly aseptic, either by the process of boiling, or by the use of an antiseptic fluid. Furthermore, it is very cheap. It can be readily used as an aspirator or as an injector, the change being made by a little device which I will show in a minute. Furthermore, the instrument can be clamped on any table, so that it will not be thrown down in the process of aspirating. The aspirator costs sixteen guldens in Vienna —that is, about six dollars and a half—which, of course, is very much cheaper than either of the regular old-fashioned aspirators. About the only disadvantage of the pump is its musical qualities.

As will be seen, it readily draws up the fluid out of the glass. In order to eject, this pump is simply turned upside-down. As will readily be seen, a good force can be

produced with very little effort. It has one little device which is useful: that is this small glass syringe, that can be put in midway; any pus or other fluid can be drawn into it, and the little syringe removed for purposes of facilitating microscopic or other examination which may seem necessary, without introducing fluid into the glass receptacle. No fluid ever reaches the pumping part, hence that part need never be subjected to sterilization. As I said before, every other part of this apparatus can be thoroughly sterilized.

DISCUSSION.

DR. S. S. BURT.—I am sure that I express the sentiments of those present in thanking Dr. Robinson for his very suggestive, instructive, and withal interesting paper.

Pleurisy has always deeply engaged my attention, and chiefly because it so well exemplifies the efficacy of physical exploration. It would be folly, however, to claim that there are not many chances for failure even in physical exploration. Only those, indeed, who are ignorant of the subject would venture to underestimate these difficulties; but the more skilful we are, and the more acute our senses, the less likely are we to commit the errors which Dr. Robinson enumerates. There are undoubtedly times when it is difficult to detect with absolute certainty a small amount of fluid in the pleural cavity. If, from any peculiarity of the chest wall, the entire thorax is somewhat dull upon percussion, there is nothing so important as a careful comparison of the two sides for purposes of differentiation. I lay a great deal of stress upon the necessity of comparison. We should endeavor to get the keynote of each individual and then explore the chest in all directions. Carelessness is nearly, if not quite, as often the cause of failure as ignorance. In the majority of instances it is a question of careful comparison and repeated examinations. No one, however, should wholly depend upon the results of a physical exploration. The history and the symptoms likewise demand consideration. But, notwithstanding this, pleurisy, and especially pleurisy with effusion, is an excellent illustration of the absolute need of physical examination, for not infrequently fluid collects in the chest with neither history nor symptoms that are in any way characteristic. The displacement of the viscera, and of the heart in particular,

is a sign of no little importance. In pneumonia the heart remains in its normal position, whereas it is always carried in some degree away from the affected side by a pleuritic effusion. There may be a lack of mobility upon the affected side in both pleurisy and pneumonia, but there is no augmentation of the chest in pneumonia, while, in pleurisy, on the side involved there is frequently an increase in the semi-circumference. We obtain in pleurisy, upon percussion, flatness, and moreover a sense of resistance that to an educated hand is very significant. The difference, therefore, on percussion, between pneumonia and pleurisy is not only one of sound but also one of feeling. We are pretty sure to find fremitus upon palpation in pneumonia, even though it be not exaggerated; but in pleurisy the absence of fremitus is a rule to which there are but few exceptions. These exceptions, however, now and then have occurred in my experience. Still, when palpation shows absence of fremitus, the presumption is strongly in favor of there being either fluid in the pleural cavity or a thick plastic exudation. In listening to the chest in pleurisy I have always found the axillary region a most satisfactory place for examination, because the lung is usually carried by the effusion away from this locality. Bronchial breathing that may be audible near the spine over the compressed lung gradually fades into absolute silence as we approach the axillary line. When bronchial breathing exists over the entire side affected, I have come to place considerable reliance upon the difference in the quality of the sound transmitted through the fluid and that which is generated directly under the ear in pneumonic consolidation. The dry pleurisies that Dr. Robinson speaks of are commonly associated with some of the various forms of friction; but this sign is frequently overlooked if the patient is not made to fully expand the lungs by deep inspiration, in order to bring into contact the visceral and parietal layers of the pleura. After due precautions of this kind I feel safe in excluding acute dry pleurisy if no friction can be generated. For the diagnosis of tubercular pleurisy we are obliged to depend very much upon the history, symptoms, and an examination of the sputa microscopically. I am inclined to think that pulmonary tuberculosis supposed to be the result of simple pleurisy is always the sequence of a pleurisy that is tubercular. The differential diagnosis between serum and pus in the pleural

cavity can be made with certainty only by exploratory puncture, but there are many symptoms, as Dr. Robinson says, which may lead us to suspect the presence of empyema. One of the difficult problems of physical diagnosis is to decide between fluid in the chest and an old plastic exudation. I have learned to depend for the solution of this difficulty upon the following points: The heart will be drawn toward the retracted side in a plastic exudation, whereas if there be an effusion of fluid the organ will be more or less displaced in the opposite direction. In fluid effusions, if the semi-circumference of the chest is not increased, it certainly will not be diminished. The resonance inclines toward dulness rather than flatness in many cases of plastic exudation. Upon auscultation vesicular breathing is heard through a plastic exudation, however faint it may be, and not the distant bronchial breathing often heard through an effusion. It is, in my opinion, entirely unnecessary to resort to the needle for the detection of fluid in the chest, and such a procedure, with rare exceptions, is a confession of ignorance of the principles of physical exploration.

With regard to the pneumonia that is associated with *la grippe*, I find, now and then, that the consolidation does not extend to the surface of the chest. In central pneumonia there are no crepitant râles, frequently there is no dulness, and possibly bronchial breathing may be inaudible. Under these circumstances I base my diagnosis upon the history, symptoms, and the inspection of the sputa, together with a development of what I have termed a "bronchial puff," by making the patient cough and breathe deeply. This forced respiration will sometimes bring out a bronchial quality that cannot otherwise be detected. Consolidation of the lung may exist without auscultatory signs, if the patient does not breathe with sufficient intensity. Weakness of pulsation and weakness of respiration render signs of auscultation in heart and lung diseases correspondingly indistinct.

In regard to the treatment of pleurisy with effusion, I am not so strongly in favor of aspiration, perhaps, as Dr. Robinson, still I have performed the operation a great many times and with excellent results. But there are undoubtedly many cases that do not require thoracentesis. Rest in bed, with restriction of fluids and a concentrated nourishing diet, are oftentimes all that is necessary for complete recovery. It is a problem of decreasing the

ingestion of fluids and increasing the excretions. Cathartics must be employed sparingly, but diuretics are less objectionable. Cod-liver oil is very useful to improve the general condition, and this alone will sometimes cause the effusion to disappear. The prevention of adhesions is facilitated, as the fluid decreases, by causing the patient systematically to practise pulmonary gymnastics. I have been in the habit of waiting two weeks before undertaking to draw off the fluid, unless it is of sufficient amount to endanger the life of the patient by pressure, and then, if there is no physical evidence of a recession of the effusion, I consider the operation advisable. While there is some risk incurred in the complete evacuation of the chest, especially if the pleurisy is not recent, yet I have frequently drawn out all the fluid with impunity. We are to be guided not so much by set rules as by the condition of the individual, for the time to stop aspirating is when the patient begins to show signs of oppression in the breathing. Always careful about the cleanliness of my needles, I have never had any trouble upon that score. Once I had a needle break and it was lost in the chest. It is well to remember the possibility of such an accident. My patient had empyema, the ribs were very close together, and, as he coughed, the needle, which was small, was broken by the ribs. Although the patient died, his death was not caused by the fragment of the needle, for it was found lying in the bottom of the chest where there were no signs of recent inflammation. After this experience I was careful to use a stronger needle; and I have found it useful to administer a little opium before operating, to lessen the tendency to cough. A similar accident occurred at Bellevue Hospital at about the same time.

I have treated empyema by repeated aspirations, and with success, but it is undoubtedly better, as a rule, to immediately make a free opening and drain the thoracic cavity. With proper drainage the chest should not require irrigation. It is not always necessary to resect a rib. I would recommend the valvular drainage tube which Dr. Phelps has introduced to the profession.

DR. E. G. JANEWAY.—I do not suppose it is necessary to go over the entire ground of physical diagnosis of pleurisy, and, if you will allow me, I will speak only of some of the rare things which I have seen cause a good deal of trouble in the diagnosis between pleurisy and other conditions—for instance, pneumonia. I have seen two cases

of pleurisy with effusion over the upper lobe of the lung, there being no pleuritic effusion below. In such cases there is very great difficulty in distinguishing between pleurisy and pneumonia. In one the diagnosis was made at the autopsy, in the other during life. Of course adhesions of the lower lobe prevented the fluid from forming there, so that it accumulated above and there flatness was found. The unusual location of the flatness, the bronchial breathing, the other physical signs, the temperature, etc., led us to think there was pneumonia instead of pleurisy. That, I think, is the most difficult thing one can encounter—pleuritic effusion limited to the position of the upper and not to that of the lower lobe.

Another thing gives rise to a good deal of difficulty and causes physicians of a great deal of experience to get into trouble in consequence of it—that is occlusion of a bronchus with pneumonia. When a bronchus becomes occluded by pressure of an aneurism or a growth, and the pneumonia takes up the whole lung, then there is very great difficulty in distinguishing between pleurisy with effusion filling the whole side, and a consolidation of the whole lung. There are methods by which one can make the distinction in these cases, and that which has given me the greatest help under those circumstances has been mensuration and the position of the apex beat of the heart. In pneumonia of the whole lung the apex beat is not displaced. In pleurisy with effusion filling the chest the apex beat will be displaced. Of course this sign is more valuable when the left side is affected, but fortunately the left bronchus is more apt to be pressed upon by an aneurism than the right. I recollect a case at Bellevue of a patient who had been shown at several clinics as an illustration of pleurisy with effusion filling the chest, and an autopsy revealed a small aneurism, not bigger than a walnut, pressing directly downward on the left bronchus, into which a small opening had taken place with very slow escape of blood from the aneurism. There the pneumonia simulated pleurisy very closely. I have seen cases since, but never as difficult a one as that. Mensuration and the position of the heart will help in deciding the case. Vocal fremitus is gone in such cases; vocal resonance is gone; whisper resonance is gone.

Another thing that gives rise to a good deal of difficulty, because we do not have the ordinary signs, occurs in those cases where fibrous exudate in the pleural cavity com-

presses the lower lobe and no free serous effusion exists. Fever, perhaps high, coexists. The case simulates a pneumonia rather than a pleurisy.

Again, there are conditions connected with the pericardium which make the diagnosis difficult. Pericarditis with a large accumulation of fluid, particularly backward, may compress the left bronchus, interfere with inflation of the lower lobe, and lead to a diagnosis of pleurisy.

Another condition which I have seen mislead very good observers is that of syphilis with periostitis of ribs. I have in mind two patients in whose cases the mistake was made. They had sweating, fever, emaciation, and pain in the side, and the diagnosis was made of phthisis with pleurisy; but they got quite well under iodide of potassium, and after the lapse of a year one weighed twenty and the other thirty pounds more than they did.

Again, we find difficulty when the pleurisy with effusion is on the diaphragmatic surface. The early accumulation of fluid there on the right side simulates enlarged liver, and on the left side enlarged spleen, and, the edges of the lung being adherent, we find no change on change of position. I have made the diagnosis, in the absence of friction, from the range of dulness and flatness.

Again, there are conditions below the diaphragm which give rise to very great difficulty sometimes—enlarged spleen held in position by adhesions from perisplenitis, etc.—so one has to be on his guard on that side, and also on the right side for diseases of the liver—accumulations of fluid between the liver and diaphragm, for instance. I can recall two cases where difficulty arose on the left side, three on the right from subphrenic pyopneumothorax. On the right side one case was dependent upon perityphlitis with abscess; two were due to suppuration following, in one case cancer of the stomach, and in the other inflammation around the pancreas. That, however, is hardly germane to our subject; but the diagnosis between abscess of the liver and pleurisy with effusion is sometimes quite difficult, particularly if we have a localized pleurisy which may be held in by adhesions, and the sign which is of most service in all those cases is that free inspiration will so depress the line of flatness in enlarged liver, spleen, and other things below the diaphragm, as to alter the percussion note, the resonance descending, the flatness ascending, with every expiration, so that we get a change with inspiration and expiration. Then, again,

the vocal resonance aids in the diagnosis. Enlargements below the diaphragm do not give rise to the vocal changes occurring as a rule in pleural effusions with serum or sero-pus.

Those are amongst the things which have given me a good deal of difficulty in diagnosis. The diagnosis of the type of the pleurisy is sometimes more difficult. One is apt to think that if a pleurisy develops in a person who has sepsis, the pleurisy must of necessity be purulent. That is a mistake. It may not be so even if the patient has high temperature. I have seen a woman have a temperature of 106° F., running up to 105° for two or three days, extremely sick, having suppuration in the neighborhood of the uterus, develop an acute pleurisy, fluid increase rapidly. I felt that the outlook for her was extremely serious; in fact, did not expect her to recover. On putting the needle into the pleural cavity clear fluid was obtained. That has also happened in the instance of a distinguished colleague, Prof. Delafield. In his case the fact that the whisper was conveyed down through the effusion pointed to the probable presence of serum instead of pus (Bacelli phenomenon), for in pus the presence of the whisper is, as a rule, lost. That fact has helped me in many cases in judging of the probable character of the fluid. An interesting case illustrating this point occurred in my hospital service in a patient whom I showed as having a probable serous effusion. The house physician drew off some of the fluid and told me it was pus. The next time I put in the needle myself and obtained clear serum. Then, on inquiring into the history more carefully, I found that the patient had had a chill, fever, sweating, and enlargement in the position of the lower ribs first, and afterward enlargement of the chest higher up. Diarrhœa had preceded the trouble. Then the needle was introduced lower down and drew out chocolate-colored pus, which was indicative of abscess of the liver. In this case the whisper enabled me to make the diagnosis of pleurisy with serous effusion, while there was also an abscess of the liver. The case is another, then, illustrating the fact that a pleurisy connected with a septic process is not of necessity purulent. The prognosis, of course, is not so gloomy after putting in the aspirating needle and drawing off serum instead of pus.

Now, with regard to aspiration of the chest, I have seen cases in times gone by, before antiseptic processes were

so well understood, in which, after aspiration, serous fluid became purulent, due to carelessness in the use of the needle. I recollect the case Dr. Burt alludes to of a needle breaking off. It happened to a house surgeon of mine. The needle did no harm. It remained in several days and was found at the bottom of the pleural cavity. The same thing happened to another of my house physicians, and this patient recovered and went out of the hospital with the needle left in. I was somewhat afraid he might have pain and come back and complain that something had been done to him which was not all right, but he did not.

With regard to the treatment of pleural effusion, it seems to me it is a rational procedure to aspirate whenever the accumulation of fluid is such as to give rise to distress or to give rise to apprehension of a fatal issue by a sudden increase. It seems to me perfectly wise and proper to draw off the effusion whenever it attains to that amount; not necessarily in a hurry during the first few days, but there is no objection particularly to doing it comparatively early, and I have never yet had in my personal experience, when operating, a single fatal issue follow directly upon aspiration by œdema of the lungs.

One thing that I have known to happen which has made the physician feel very uncomfortable has been the occurrence of hydropneumothorax following aspiration. I have known this to come about in cases, not by puncturing the lung, but by the fact that there had once been pneumothorax which was followed by hydropneumothorax, the fluid then accumulating to such an extent that the lung became compressed, collapsed, and there was no room for more air. But after aspiration of the fluid the air came out again through the opening into the pleura from the lung. The opening had been occluded by fibrin, probably. That fact should be borne in mind, for I have known a physician to be blamed for having improperly performed aspiration, on account of hydropneumothorax appearing afterward. It does not necessarily mean puncture at the time. The history may establish the fact that the patient had had hydropneumothorax, the pneumothorax having disappeared with the accumulation of fluid.

Now, again, with reference to the quantity of the fluid to be withdrawn, I agree with the statement that we do not want to draw all off at once. There is no necessity for it. We take off usually what the patient can comfort-

ably stand, and as soon as he feels oppressed stop the aspiration; or I may draw off, say, two quarts, and, though the patient has no distress I may think it better to stop and aspirate again later. As to the point of aspirating, it can be done in the back or side of the chest. Before aspirating one should measure the chest and locate the apex beat of the heart, for he may put the needle into a thickened pleura or consolidated lower lobe. I have known physicians to be bothered because they did not get fluid on puncture, whereas measurement of the chest would have explained the whole matter, showing that the case was one of chronic pleurisy with adhesion.

I have tried the dry treatment a number of times and had good results. It does not do well, where the patient has much fever, to restrict the fluid taken too much, but it can be regulated by giving only two-thirds as much fluid, for instance, as is voided with the urine. The patient may be given iodide of potassium and salt in capsules, which will make the blood a little more thirsty for the fluid in the pleural cavity, and sometimes help along absorption considerably. I have seen much more rapid absorption after aspiration aided by the dry treatment.

Then I think that in drawing off a small quantity of the fluid the greater part of the benefit is obtained by acupuncture. The outer opening closes, but the inner opening remains patent and fluid exudes; and where a number of punctures are made, only a little fluid being removed at a time, absorption may be hastened, the result of acupuncture of the pleura. I have tried another procedure in some cases, and in some with very good results, and that is to throw creosote into the pleural cavity where there was a probability of tubercular pleurisy. I have tried throwing in two to five drops in alcoholic solution. No harm has resulted from it, while in some cases there has been much improvement. Whether the patients were really affected with tubercular pleurisy I cannot say. I have had one case with peritonitis and pleurisy. The patient had sweating, high temperature, rapid loss of flesh. There was no improvement of the peritonitis, and after a week she developed double pleurisy. The creosote was injected into each pleural cavity, and afterward the fever dropped, absorption of the pleural effusion took place, the peritonitis disappeared. Had it not been for the success obtained the case would have been regarded as undoubtedly tubercular. Since then I have tried it in

another case with benefit. I have tried it in rheumatic pleurisy, but without satisfactory results. Salicylate of sodium has done better in rheumatic cases. In the treatment of pleurisy we have to adapt ourselves to the condition we have to deal with, and treat the patient as well as the pleurisy. We do not want to forget the patient's condition and the associated diseases when we are treating pleurisy.

The question of operating for empyema I think is very well established. Nothing special is to be added upon that subject.

I will say one word, however, upon the question of tuberculosis following pleurisy. I know of no matter more difficult to judge of than whether a previous attack of pleurisy with effusion, occurring in a person who afterward showed tuberculosis, was tubercular in nature. I have seen within the past year three persons with tuberculosis who had had attacks of pleurisy with effusion on one side seven, six, and five years ago, recovery having taken place from the pleurisy. One of these persons has died; the other two have phthisis now. In two the phthisis manifested itself at the apex on the opposite side from that on which the pleurisy had occurred. We cannot say that the subsequent manifestations of tuberculosis point of necessity to the pleurisy having been tubercular. There is no reason why in that time these three people should not have received the germs of tuberculosis and become affected as any one else might who had never had pleurisy. It does not do to argue that because one thing follows another the first was necessarily the cause of the second, or that there was a direct association.

DR. A. L. LOOMIS.—Dr. Robinson has brought before us a very interesting subject, and one which can hardly be properly discussed in one evening. The two points which Dr. Robinson has especially considered are its diagnosis and treatment. Before very much can be said in regard to the diagnosis of pleurisy in any given case, it is important to determine the quality of the pleurisy under consideration, whether it is an acute or chronic fibrinous pleurisy, an acute sero-fibrinous pleurisy, or an acute suppurative pleurisy. The diagnosis of pleurisy by the physical signs alone is faulty. No man can reach that height of experience in physical exploration that he is able to diagnosticate all pleurisies by the physical signs alone. It seems to me that in every case which is ob-

scure, and even in those that are not, it is important that the history shall be very carefully considered before physical exploration is attempted. In a large proportion of cases one will be able to reach a diagnosis by the history alone, and will be able to exclude those other diseases which are liable to be confounded with pleurisy. In an acute suppurative pleurisy, ushered in by chill, high temperature, and the other symptoms common to an acute infectious disease, the same as in an acute pneumonia, we are compelled to rest mainly upon our physical signs in order to make our differential diagnosis. An acute plastic pleurisy has no such history, and is easily distinguished by its history alone from an acute pneumonia. While in many cases one is only able by the history, in connection with the physical signs, to make the differential diagnosis, in a certain proportion of cases it may be made without the history, or by the history without the physical signs.

Now, in the obscure cases—those cases difficult of diagnosis—it seems to me that if one studies them carefully, taking the clinical history in connection with the physical signs, he will very rarely make a mistake. Take, for instance, the question Dr. Janeway has just raised in connection with pressure upon the left bronchus by an aneurism. I have seen a number of such cases, but they were of gradual development. The pressure did not take place at once on the bronchus sufficient to obstruct it, but after the signs of pressure the signs of pulmonary consolidation developed. If the pressure completely obstructs the bronchus, consolidation of the lung results; then physical signs resemble very closely those of pleurisy with effusion.

Dr. Janeway.—I referred to cases of acute pneumonia consolidating the whole lung.

Dr. Loomis.—I am referring to cases in which pressure of an aneurism upon the left bronchus leads to consolidation of the lung. The consolidation in these cases, as far as I have watched them, takes place slowly, not suddenly, and the history will lead you to the diagnosis. If you do not see the case until after the pressure has completely obstructed the bronchus, then the signs will tally very closely with those of pleurisy with effusion, and there may be difficulty in diagnosing by the physical signs alone.

Cases of pleurisy that have given me the greatest anxiety

and uncertainty are the acute plastic pleurisies in which there is a rapid and large plastic exudation, in which you have the physical signs, bronchial breathing, the voice signs, and in fact all the physical signs, except fremitus, of pulmonary consolidation. The absence of tactile fremitus is the only physical sign which determines the diagnosis in such case. If you have not the history in such cases you would perhaps be misled; but taking the history with the physical signs, you will usually be able to make the diagnosis. The absence of expectoration, the hacking cough, the low range of temperature and the pulse, lead you to pleurisy, but the physical signs resemble very closely those of consolidation of the lung. Often such signs are present in acute plastic pleurisy for a considerable time, and then they will gradually subside and suddenly you will have the pleural cavity filled with fluid. Those are the cases, it seems to me, in which it is most difficult to determine whether it is best to aspirate or not in acute pleurisy. Another question will also come to you in these cases of large plastic exudation: whether they are not tubercular. Oftentimes the history of the case will be such as to lead in that direction.

I will refer to a single case which gave me a good deal of trouble. A young girl, eighteen years of age, of strong phthisical heredity, who had had enlarged cervical glands during the early summer and spring, who had become very much run down, had phthisical aspect and a little, hacking cough, after two or three months spent in the Adirondacks came home in the fullest vigor, had never looked as well in her life as then. After horseback riding here in the city one day she was exposed to cold and had an acute pleurisy, with the physical signs to which I have just referred—bronchial breathing, etc. After a slight subsidence of the acute symptoms for a week, suddenly the left pleural cavity filled with fluid. The question arose, Is she safe without aspiration? The heart was pushed far over to the right by movement; the left chest was over an inch larger than the right. I determined, on account of similar cases that I had seen, that it was better to wait. I did wait, and in two weeks the fluid entirely disappeared, in two months all physical signs of pleurisy had disappeared from her chest, and she is in a better condition now, at the end of four months, than at the time when she returned from the Adirondacks.

Another class of cases which have given me a good deal of trouble in diagnosis are the chronic forms of circumscribed empyema: those cases in which you have pus collected either at the apex, perhaps, or in the axilla, or in some portion of the chest—circumscribed areas of fluid. Oftentimes those cases will go on unrecognized until a large expectoration of pus occurs, an opening having taken place into a bronchial tube. Those, to me, are the most difficult cases. Often you may introduce the needle into the chest, and unless you are fortunate in the exploration you may not strike pus.

As regards those dry pleurisies which we are every now and then meeting, to which Dr. Robinson referred as rheumatic and gouty, occurring in the fibroid diathesis —those attacks of pleurisy, it seems to me, are quite frequent, and they are always characterized by the physical signs of friction, so that there can be no difficulty in recognizing their existence. There is simply an interstitial pleurisy which gives you very little if any physical signs, and afterward you make the post-mortem and you find the adhesions and perhaps thickening of the pleura.

There is another form of dry pleurisy which means more. It is a form which occurs at the apex of the lung, and which exists over a considerable portion, and is accompanied by a dry, hacking cough; and after the cough has gone on for some time, perhaps, the patient will have a hæmorrhage, but all the physical signs you get at first are those of dry pleurisy at the apex of the lungs. Now, I believe those pleurisies are usually tubercular, and from recent investigations which have been made in the histological department of the Laboratory by Dr. Coakley, to determine the lymphatic system of the lungs, he traced a direct communication from the bronchial glands to the upper portion of the pleura at the apex of the lung; and if we believe that tubercular bacilli enter through these glands, it is very easy to see how they may find lodgment first in the pleura at the apex of the lung and give rise to this pleurisy. Those, to me, are very interesting cases. Every one of the gentlemen here, undoubtedly, has had cases of dry pleurisy at the apex of the lung exist a long time before there were any other physical signs or any other rational symptoms of phthisis.

Dr. R. C. M. Page.—The hour is so late, and the subject has been so thoroughly and ably discussed, that there is but little left to say, and I will be very brief. Now,

with regard to diagnosis, although it is usually very readily made, of course difficulties do arise. For instance, Walshe describes a case of enlarged heart that was mistaken for pleurisy with effusion. I remember that some twenty years ago a case was exhibited at Bellevue Hospital of thoracic aneurism mistaken for a typical case of pleurisy with effusion; and six or seven years ago Dr. Hudson had a case of enlargement of the spleen upward instead of downward, which was mistaken for pleurisy with effusion, the diagnosis only being made by dry puncture with the hypodermic syringe. It seems to me, sir, we always have that method to fall back upon, and where it is done properly, with care, and only when occasion warrants it, we are justified in doing it. Not that I want to make light of entering the chest with needles. I think it is a matter we should look upon always seriously, and never do it unless compelled to.

It seems to me a proper and convenient classification of pleurisy would simplify the matter of treatment, and I have usually regarded it as of three distinct types: 1. Dry, circumscribed pleurisy; 2. Pleurisy with serous effusion; 3. Suppurative pleurisy.

I have never had occasion to use aconite or veratrum in pleurisy, though it would evidently be much less dangerous than in pneumonia, where a much greater tax is put upon the heart. To promote absorption in ordinary cases of so-called rheumatic pleurisy, sodium salicylate is the best remedy I have ever tried. It seems to me it acts very much as it does in the effusion of articular rheumatism. In subacute cases nutritious diet, tonics, counter-irritation, and moderate exercise are indicated. I have never obtained any benefit at all, I think, in treating cases of pleurisy with effusion by means of iodide of potassium. In the syphilitic cases mentioned by Dr. Janeway there is no doubt about it that iodide of potassium is the great remedy, but, as far as getting rid of the effusion in ordinary cases is concerned, I have never seen any good from iodide of potassium. It is apt to upset the stomach and take away the appetite.

The question of aspiration comes up in the course of the disease. It is not only important to determine when and in what cases it should be done, but I desire to emphasize here that aspiration of the thorax should always be regarded as a serious matter, for we are to avoid adhesions as far as practicable. For this reason aspiration

should not be performed during the period of acute inflammation—during the first three weeks, say—unless imperative, in order to save the patient's life for instance. In such conditions it is not only justified, but it becomes an absolute necessity. There are about five conditions in which we are not only justified in aspirating, but in which it becomes absolutely necessary: 1. Great amount of fluid threatening syncope from pressure; 2. When the amount of fluid was not so extensive, yet gave rise to fits of orthopnœa independent of such complication as bronchitis; 3. When the fluid occupied half a pleural cavity and showed no signs of being absorbed after a month; 4. In double pleurisies where the total amount of fluid would about fill one pleural cavity; 5. In all cases of pus.

So far as the point of puncture is concerned, I will say that in those cases in which I have punctured in the scapular line low down, owing to the little flocculi in the fluid the needle becomes stopped very often, and it becomes quite tedious to be taking the needle out to clean it or to use a fresh one, and for that reason I usually prefer the posterior axillary line, going up a little higher. There you can withdraw all the fluid necessary without running the risk of getting the needle plugged. It is quite a point in performing these operations in ordinary cases. After the patients have got around so as to be able to come to you out of doors, I have always insisted on their going to some hilly country where they can take exercise, walk up and down hills, compelling them to expand the lungs and thus avoid any possibility of adhesions, and, failing that, I should recommend the pneumatic cabinet.

Dr. A. H. Smith.—Among the difficulties which may arise in the diagnosis there is a condition which I believe has not been mentioned this evening, and which is the result of a previous adhesion of the lung at some point to the chest wall, this adhesion preventing the lung from rising to the top of the fluid when an effusion subsequently takes place. In such cases as this, if the effusion is sufficient, you may have the lung entirely surrounded by fluid except at the point where it adheres to the costal pleura. Now, this brings about a peculiar condition of the physical signs. The parenchyma of the lung is condensed, but the bronchial tubes may not necessarily be so entirely compressed as to prevent air to some degree passing into them, and we therefore have the physical signs of pneu-

monia very closely simulated. The bronchial breathing is transmitted through the fluid to the ear and is very distinctly heard. We may also get vocal resonance. But the one point which Dr. Loomis has already called attention to, and also Dr. Janeway, that will help us out, is that in these cases we generally do not get vocal fremitus. The vibration which is produced in the bronchial tube is not sufficiently powerful to be transmitted through the fluid in such degree as to be felt by the hand when applied to the chest. I have very recently had a case of this kind where the advantage that might be derived from the history was excluded, for the man was a Syrian and could not speak a word of English or any other available tongue. He was brought to the hospital with nobody to give a clue to his previous history, except the fact that he had been more or less ill for about two weeks. I found over the right side complete dulness, or almost flatness, to the very apex of the chest, pronounced bronchial breathing, and vocal resonance, but absence of fremitus. Mensuration established a difference of about three-quarters of an inch. The heart beat was very slightly displaced. The introduction of the needle gave serous fluid, the aspiration clearing up the case completely. There was considerable fever in this case, and the patient had many appearances of pneumonia, but still was not ill enough for so extensive pneumonic consolidation.

We sometimes find physical signs present which it is difficult to account for. Only a few weeks ago a girl was brought into the hospital with a diagnosis of pneumonia. I do not remember now about a chill, but at any rate she had considerable fever, and at the lower part of the left chest there was dulness on percussion, distinct and typical, and fine crepitus, limited to inspiration, as typical as I ever heard in any case of pneumonia. But there was absence of fremitus, and I suspected that there was fluid there, and the introduction of the needle showed my suspicion to be correct. Now, I am entirely at a loss, on any principles of physics which I am familiar with, to account for the presence of fine crepitus over the fluid. It certainly was not produced by the lung in that situation. It was not heard above the level of the fluid. In fact, above the level of the fluid we had œgophony. It could not be due to any attrition between the pleural surfaces, for the surfaces were evidently separated from each other by a considerable amount of fluid. I simply give it up. I have

the courage of my uncertainty, as men sometimes have the courage of their convictions. I am willing to say in this case I do not know the cause of it.

One little practical point with regard to aspirators. We are sometimes driven to the wall from the absence of more formal methods, and the exigencies of frontier service in the army some years ago brought me to the invention of an aspirator which has served me frequently where I could have had a better one if I cared to, at least a more sightly one. If you take a quart bottle, adapt a tightly fitting cork to it, pierce this cork with a bit of glass tube, put over this glass tube one end of a rubber hose, and attach to the other end of the hose a hollow needle through which the fluid can flow, you have all the essentials of a perfect aspirator. All you have to do is to put about a drachm of ether into the bottle, set the bottle in a pail of hot water, wait until the ether has fully filled the bottle with its vapor, then remove the bottle from the water and introduce the needle. The moment the vapor begins to cool you have a vacuum produced, and you can draw out nearly the entire volume corresponding to the contents of the bottle, the vacuum being almost perfect.

Dr. B. Robinson.—I should be most happy to take up each one of these questions or points which have been mentioned by the gentlemen who have been kind enough to discuss my paper, but it is late and probably it is better not to do so. I will simply say, in concluding this evening's discussion, that what I particularly wished to direct attention to in my paper was not, so to speak, the differential diagnosis between pleurisy with effusion and certain more or less rare complicating conditions that might possibly occur, with the presence of effusion or not; what I wished especially to do was rather to show, in instances where we had very little doubt of the probability of pleurisy as compared with any other lesion, how frequent it was to find that the very signs that we should place most reliance upon were at times absent or incompletely expressed, so that while we recognized a pleurisy was no doubt there, and it was proven afterward it was there, yet if we referred to the authorities as our guides for the purpose of making such a diagnosis we would find them somewhat at fault. I would like merely to add with reference to some diseases which are so accurately described in almost every classical text book, even in the later editions, that often they are not in accord with

clinical experience. I think a good many of the text books have been written in accord, perhaps, with what the author thought it his duty to put into the book, and in accord with what the authorities taught. Now, authorities, in my judgment, are occasionally inaccurate, even with reference to a disease so common as pleurisy.

Special Meeting, March 9th, 1893.

REPORT OF A RECENT SANITARY INSPECTION OF ONE OF THE SOURCES OF THE CROTON WATER SUPPLY.

By TIMOTHY MATLACK CHEESMAN, M.D.

Mr. President and Fellows of the New York Academy of Medicine:

It gives me much pleasure to respond to your invitation, tendered through the "Committee on Conference with the Board of Health" and the President of the Academy, to lay before you certain facts gathered from a recent sanitary inspection of some of the sources of the Croton water supply.

In the Report of the New York State Board of Health for 1889, and in that of the Health Department of the City of New York for 1891, it was shown conclusively, at the times these reports were made, that there existed on the water-shed numerous sources of pollution of the Croton water.

Within ten days (28th of February, 1893) Dr. Martin, chemist to the Health Department of this city, in an address before the "Section in Public Health, etc.," of this Academy, gave facts, learned in an official capacity, which confirm in the strongest manner our belief in the unsanitary condition of the sources of our water supply.

In the first report, from inspections made in December, 1884, we find that the privies of two hotels accommodating one hundred and fifty guests, one privy in a shop and two in dwellings, five urinals, and a small slaughter house drained directly into Tonetta Brook, a tributary of the east branch of the Croton River; and

that two privies in the railroad depot in the town of Brewster are immediately adjacent to the brook.

In the second report we read that about eighteen months later (August, 1891) five privies existed on the bank of the brook, six were within ten, twenty, forty-five, and eighty feet of the brook, sewer pipes from sixteen houses and a urinal from a bank discharged immediately into the brook, and that other sources of pollution existed, such as dwellings adjacent to the brook from which slops were thrown out, barns, poultry yards, etc.

These two reports show that the nuisances observed in this locality in 1884, and reported in the publication of 1889, still existed in August, 1891.

From time to time during the past summer we were informed, in a general way, that the nuisances existing on the Croton water-shed were being abated, so far as it was possible to abate them.

The Report of the Department of Public Works for the quarter ending the 30th of September, 1892, published in the *City Record* on the 24th of February, 1893, states:

"The Department has for years been hampered and obstructed in the performance of one of its most important duties, the protection of the city's water supply from contaminations by nuisances on the borders of the streams, lakes, and ponds in the Croton water-shed.

"Recent legislation on the subject has had the tendency to protect the people living in the Croton basin in maintaining old nuisances, rather than the ostensible object of strengthening the hands of the city authorities in preventing pollution of the water. The threatened epidemic of cholera made it necessary to adopt energetic measures to remove or abate such nuisances as far as possible, notwithstanding the obstructive legislation. In the Report of the Chief Engineer of the Croton Aqueduct will be found a detailed description of two hundred and twenty-six such nuisances, consisting of privies, pig-pens, cattle-pens, manure heaps, etc., which have been removed during the past quarter. The beneficial results of this work are

SING SING
MERTENS
HITCHAWAN
CROTON LAKE
YORKTOWN
AMAWALK
W. SUMMERS
BALDWINSVILLE
MAHOPAC MINES
MAHOPAC
CRAFTS
CARMEL
TILLY FOSTER
BREWSTER STA.
CROTON WATER SHED
PUTNAM COUNTY
DUTCHESS COUNTY
NY AND NEW ENGLANDRA
CONNECTICUT

PLATE I

shown in a chemical analysis of the Croton water near the close of the quarter, which proved the water to be free from nitrates and other ingredients which would endanger the public health."

From the Report of the Chief Engineer we learn that in Brewster seven privies were removed and replaced and furnished with cemented vaults; one hundred and sixty loads of garbage and one hundred and fifteen loads of manure were removed; a slaughter house and a dam were cleaned out, and all garbage was cleaned out from Tonetta Brook, and lime was thrown along the bank for eight hundred feet.

In connection with a series of studies of Croton water, I made visits to several parts of the water-shed to ascertain the extent to which these nuisances had actually been suppressed by the authorities, and to learn what sources of pollution still existed. Several of the visits were to Brewster, and there a careful inspection of Tonetta Brook was made. The condition there found I will endeavor to outline from a series of photographs made on the 20th and 21st of January, 1893.

Plate I. (Photograph of portion of a map published by the New York & Northern Railroad.) This is a plan of the Croton water-shed. Most of this water-shed lies in Dutchess, Putnam, and Westchester counties in the State of New York, and only a small portion extends into the State of Connecticut. Above Croton dam the water-shed drains an area of about three hundred and thirty-eight square miles; an increased drainage area of about twenty square miles will be added after the completion of the dam at the Cornell site. The population of this area is estimated at twenty-five thousand, and about thirty-five thousand cattle are maintained within its boundaries.

The west, middle, and east branches of the Croton River, together with the lakes and the reservoirs now existing or in course of construction, are indicated. All the water which passes through the old and new aqueducts enters

Croton Lake, which serves as a distributing reservoir to New York City.

Our particular interest centres in the region around Brewster.

Plate II. (Photograph of a map published in the Report of the State Board of Health, 1889.) This is a map of the village of Brewster. The east branch of the Croton River flows south of the village, and a portion of Tonetta Brook flows through its western portion and empties into the Croton River at the point indicated.

The population of this village is somewhat over three thousand, of which number about one thousand live along that portion of Tonetta Brook shown on the map. The natural slope of the land adjoining these water-courses is such that, directly or indirectly, the drainage of the village is received by the east branch of the Croton River or by the small tributary mentioned.

We will first view the general lay of the land on the portion of the east branch of the Croton River south of Brewster, and then proceed with a careful inspection of its tributary known as Tonetta Brook.

Plate III. This is a view of the district lying to the north of the Croton River in the village of Brewster, and shows a populated area which drains into the Croton River.

Plate IV. gives a view of the same district from the west, and is instructive as illustrating the alignment of privies north of the stream.

Plate V. shows a condensed-milk factory situated upon the bank of the Croton River, and indicates the facility afforded for emptying waste fluids directly upon the banks of the stream. Some of the privies belonging to the establishment are immediately upon the east bank of the river. I understand that these are used at the present time only as urinals.

Plate VI. shows the east branch of the Croton River in the foreground, which often brings from the Bog Brook and Sodom reservoirs a considerable percentage of the

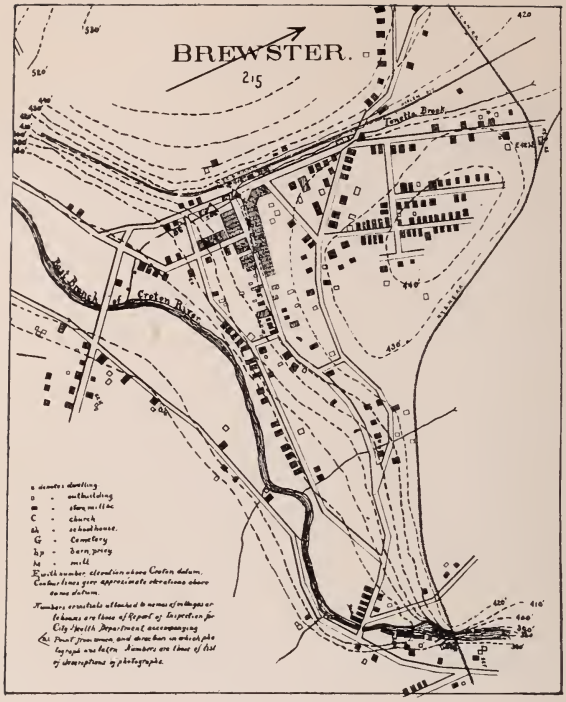

PLATE II.

PLATE III.

PLATE IV.

PLATE V.

water used in this city. A daily amount of 20,000,000 gallons is required in this portion of the river to supply the factories, but more than 150,000,000 gallons a day have been drawn from these reservoirs and led through this channel for the supply of New York City. The mouth of Tonetta Brook is shown on the left, covered with snow and ice, and on the right is a part of the village of Brewster.

A short distance up the brook on the left is a dwelling ten feet from the west bank of the brook, and I have seen evidences of garbage and slops having been recently thrown out from the porch. A privy formerly located on the bank has been removed. On the opposite side of the brook is a dwelling, thirty feet from the east bank, from which slops are thrown out. A privy and poultry house formerly on the bank have been removed.

The privy belonging to the house last mentioned is forty-five feet from the brook and has a cemented vault; an adjoining privy about ten feet further removed has an uncemented vault. A line of privies facing the east branch of the Croton River are on ground which slopes toward the stream. A drain pipe from the cellar of a house on the east of the brook opens from the wall near the bridge.

Beyond the bridge, on the right, is a dwelling thirty feet from the east bank, from which a dump had been established and was seen upon the ice overlying the brook. A privy, formerly but ten feet from the brook, has been moved about fifty feet further back and is provided with a cemented vault; another privy, belonging to an adjacent house, has a cemented vault and is also about sixty feet from the bank. Two poultry yards, reported in 1891, were not found. A refuse heap near the privy last mentioned showed, on the 3d of February, indications of washing directly into the brook from the bank on which it was located.

Following up the course of the brook, we find that it passes through a series of uncultivated fields for several hundred feet. In the Report of the Health Department

for 1891 the northern part of this area, adjacent to the brook, was spoken of as "apparently the town dump." It has now been cleared up and its use as a dumping ground has been discontinued.

On the east bank of the brook is an old boiler house and a grist mill. The mill is run by brook water and has a runway overhanging the brook. The land on the east adjoining the brook is used for truck-gardening. Somewhat further to the east, seventy-five feet from the brook, is a small barn with manure scattered about.

Plate VII. is a view from the north side of the mill. It shows a sewer pipe opening from the east wall of Tonetta Brook, beneath the bridge, and discharging its contents immediately into the brook. I am informed that this pipe received the sewage from a hotel on the north side of the main street of the village and six buildings on the south side, besides surface water from the street. Five of the buildings on the south side have been recently burned, but the hotel and one remaining house still use this pipe as a means of discharging their filth.

A view of the rear of the houses just mentioned, now in ruins, is given in the report of 1889. It shows the brook adjacent to the wall. In the cellar of the nearest building was a bottling establishment, which discharged its washings through a pipe seen in the wall. The surface drainage from the yard finds its way directly into the brook.

A view of an adjoining locality, given in the Report of the State Board of Health (1889), shows four sewers emptying directly into the brook as it passes through a culvert beneath the street. The arrangement of the pipes has been somewhat changed, but they still empty into the brook. From two of these pipes are discharged the sewage from a row of houses, including a hotel on the north side of the main street, and the sewage from the privy vault beneath a bank.

Plate VIII. shows the north end of the culvert and the brook streaming into it from the left. The surface drain-

PLATE VI.

PLATE VII.

PLATE VIII.

PLATE IX.

age from the street flows freely into the brook at this point. The building in the foreground is the bank referred to, and on the right is seen a recently erected carpenter shop with several small pipes passing out through its foundation. Their use I did not learn.

Plate IX. Here we see the brook emerging from beneath the railroad station. Two privies exist in this station, and they are located in the projecting part of the building on the east side. The vaults are said to be of cemented stone. How these vaults are cleaned out was not apparent. Within the station the door marked "For Ladies" was kept unlocked, and the only time I had the boldness to enter there were infallible signs present of recent use. The door marked "For Gents" is kept locked, but the key is with the agent, and the privy was used twice in one afternoon during a half-hour which elapsed while I was waiting for the train. Beyond the station in the picture is shown the freight house, located over the brook, and, beyond this, a receiving yard for cattle in waiting to be shipped by train. I have seen the surface drainage from the street pour in streams into the brook at these points.

The brook now runs along the side of the railroad tracks for three hundred or four hundred feet. On the left or west bank of the railroad is a dwelling, and on the right a street drain emptying directly into the brook. The dwelling, from which slops are thrown out, is situated ten feet from a tributary of Tonetta Brook. The privy is about forty feet away. At times the bed of this tributary is dry, but at other times it carries a considerable amount of water, which empties through a culvert, with the drainage from the westerly side of the railroad, directly into the brook.

The drain pipe on the east side leads the surface drainage, from the street above, directly into the brook. That part of the village lying to the east of this point is situated upon the side of a hill and is quite thickly populated. The surface water is carried in open drains along the side

of the street to the street below, whence it is conducted into the brook by the twelve-inch drain pipe mentioned. Upon the road in front of most of the dwellings in this district garbage heaps have been piled, and were being slowly washed away during the thaw and carried directly into this tributary of the Croton River.

The purpose of this drain pipe escaped me at first, as the mercury had been below 20° Fahr. for some time and no water was passing through it. On the 3d of February, as I was inspecting the drain of a privy on the bank, a small inhabitant of the village, who was evidently accustomed to health inspectors, called out, "Say, Mister, here is what you are looking for," and led me to a point of vantage to view the drain pipe.

Further to the north, the brook runs along the westerly side of the foundations of a dwelling house. The privy is ten feet from the bank, and the vault is said to be cemented. A poultry house with yard in the rear is within a few feet of the brook. I have seen a considerable garbage heap, and a dead fowl upon the ice covering the brook.

Plate X. This house, as will be seen, is built over the brook. A garbage heap was apparent under the bridge. The privy of the house in 1891 was immediately on the west bank, but has now been moved about twenty feet away and has a cemented vault.

Plate XI. We find here a blacksmith shop built over the brook. A trap door is said to have served the purposes of a privy to the patrons of this establishment, but objections to the practice were made by those guarding the purity of our drinking water, and a cemented privy vault was built on the hill to the left. On the 3d of February this vault was surrounded by a "trackless expanse" of snow nearly a foot deep, and on inspection it showed no signs of having been used.

Beyond this point were found a factory upon the west bank, not used at present, and two dwellings in the distance, a barnyard and two privies eighty feet from the

PLATE X.

PLATE XI.

east bank of the brook, and two dwellings, one thirty feet from the west bank and one upon the east bank.

A small slaughter house some distance up the stream was not photographed, but it is pictured on page 380 of the Report of the State Board of Health for 1889. It is still used as a slaughter house, and drains its putrefying refuse directly into a tributary of the brook about one hundred yards away.

From the inspection of the locality drained by this small tributary of the Croton River at Brewster, it is evident that the efforts made at the time an outbreak of cholera was feared during the past autumn have resulted in improvement. In this region most of the privies reported by the health boards as existing upon the banks of the brook have been abolished or moved back and furnished with cemented vaults. Of the privies further removed from the stream, some have been moved still further back and some have been furnished with cemented vaults. In addition to this, three poultry yards seem to have been abolished, and the field adjoining the brook, "apparently the town dump," has been cleared up.

The following nuisances, however, still exist: Three sewer pipes receiving the filth from two hotels and several houses empty directly into the Croton water. Of the eleven privies found in this region, the two in the railroad station and one adjoining a stone yard are on the bank of the brook, and the others are further removed—one ten feet, one twenty feet, one forty-five feet, three sixty feet, and two are eighty feet away from it. Of the ten dwellings near the stream, one spans the brook, and the foundation wall of another forms its eastern boundary, two are ten feet away, three are thirty, one is forty, and one is seventy-five feet distant.

In addition we find the railroad station, a freight house, and a blacksmith shop built over the brook, and a grist mill, a carpenter shop, a cattle-receiving yard, and a factory immediately adjacent to it.

To this list of nuisances, some of them sources of direct

pollution of our drinking water, we must add the surface drainage of the whole region adjacent to the stream. This gathers the animal droppings and refuse from the streets and carries them, with the house slops and garbage thrown out from many dwellings, directly into the brook.

In this locality, then, the banks of one small tributary of the Croton River, we find the principal pollutions to be from sewer pipes, privies, dwellings, and surface drainage.

It seems hardly necessary to prepare an argument to convince any one that a brook is polluted by sewage running directly into it.

That water in the vicinity of vaults and cesspools used for the reception of human excreta is polluted, has been proven beyond question. How far the contents of a privy vault may spread through the soil, I will illustrate by an extract from a Report of the Board of Health of the City of Minneapolis (1888, page 30). The Health Officer was asked to examine the soil of an excavation, 45x150 feet and 12 feet deep, upon which a building was to be erected. He found "in the centre the site of an old privy vault and cesspool, and for the entire distance each way the ground was saturated with stinking filth. How far beyond these limits the pollution had continued it was impossible to state; but the water taken from a well nearly two hundred feet distant was putrid." The same writer states "that since then he has been frequently called to visit excavations for buildings where the workmen have struck veins of polluted earth, the stench from which was almost unbearable. On several occasions, being curious to see and know the cause, he followed the veins and found that they had their origin in old cesspools and privy vaults and their outlets were into wells varying from forty to two hundred feet distant. In its passage the liquid matter from the pits had defiled the earth for several yards in all directions."

The object in cementing a privy vault is to prevent the escape of its contents and thereby prevent the defilement of the surrounding soil. In order to accomplish this ob-

ject the vaults must be made and maintained absolutely water-tight. It would be interesting to learn how many of the cemented privy vaults scattered along the banks of the water-courses on the Croton water-shed fulfil the end for which they were constructed. But we have no means of finding out whether they leak or not, as they are buried in the earth. Another manner in which the contents of a cemented privy vault may escape is by overflowing. To prevent this the surface water must be kept out of it and the contents must be removed at proper intervals. A cemented privy vault is likely to be a false security against soil pollution, unless it is so placed as to allow of a thorough and frequent inspection, and the repair of any leaks or fissures that may occur.

House-slops in the country are not infrequently thrown out upon the ground at the back of the house. My observations in the neighborhood of Tonetta Brook lead me to the conclusion that this custom was there a prevalent one. The word house-slops, as used here, means any of the fluid or semi-solid refuse of the household, whether it be waste water from the kitchen or elsewhere, or the contents of vessels that receive urine and occasionally fæces which are passed in the house. Especially in cases of illness are the house-slops likely to be mixed with excrementitious matters, and this material, which may contain countless germs of disease, is likely to be washed by rains from the ground where it is deposited and carried with the surface water to some well or water course.

Tests frequently show a large increase in the number of bacteria present in running waters after a heavy rain or thaw. This increase is largely due to washings from the banks. The vast majority of species found are harmless to the human economy, and many of them act as purifiers of the water by breaking up organic matter washed into the stream. But surface washings from a polluted bank are quite as likely to carry in the bacteria of disease, should they be present, as bacteria that do not possess the power of doing harm.

That this danger is a real one is shown by the story of an outbreak of typhoid fever in 1888 in Providence, R. I., which has been traced to such an origin. The discharges of a little group of typhoid-fever patients, thrown upon the bank of the Pawtuxet River, were washed into the stream by heavy rains. At the pumping station, three-and-one-half miles below this point, the water is pumped directly into a reservoir, from which it flows by gravity into a second reservoir and then enters the city mains. It may seem incredible that so small a pollution should cause such serious results, especially when we consider the fact that the daily flow of the river at the time the disease germs were washed into it was estimated to be six hundred million gallons.

Chemical and biological tests both show that the water gathered on the Croton water-shed is naturally a very good water indeed; yet we have proof of sewage pollution of the water, which at any moment may be a source of danger. The district just examined is said to be the worst source of pollution of the water on the water-shed, and yet it is recently reported that "the drainage of thirteen barnyards, a score of privy vaults, the garbage and refuse from about the same number of private residences, and two hotels, emptied directly into Lake Gleneida, and this wholesale contamination entitles Carmel to rank only sixth in importance as a source of pollution" of the Croton water.

So long as sewage pollution exists on the water-shed the Croton water must be regarded as liable to infection with germs of disease, and to become at any time an absolute danger to the public health.

Experiments have demonstrated beyond peradventure that the bacteria of certain diseases do live in water, and, what is more to the point, the bacillus of typhoid fever and the spirillum of Asiatic cholera have been, more than once, detected in waters which were suspected of spreading these diseases.

It is claimed that the process known as the self-purifi-

cation of water renders a polluted water again pure and fit for domestic use.

"But the process of oxidation and sedimentation, which, as the most exact chemical analysis demonstrates, are capable of freeing water in lakes and running streams from the organic compounds abundant in sewage, have but little bearing upon the actual producers of infectious diseases, such as typhoid fever and cholera. These germs are living things, and are not demonstrable in water by chemical methods, nor are they with any certainty removed from sewage-polluted lakes and streams within the limits which chemical experiences have led us to regard as safe.

"Sedimentation does remove many harmful bacteria from sewage-polluted waters. Dilution does diminish the chances to incur disease for every consumer. Many individuals are, at favored times, practically invulnerable to the incursions of these tiny foes. But it is safe to say that without purification sewage-polluted water is not fit for men to drink, no matter how fast and how far the river runs, or how wide the lake into which the sewage drains. With the size of the lake and the volume of the river the chances of harm decrease, of course, but they stay chances still where none need be."

The British Local Government Board emphasized the danger of water pollution in its cholera circular of the 5th of July, 1884, as follows: "First, and above all, there is the danger of water supplies which are in any way (*even in the slightest degree*) tainted by house refuse or other kinds of filth, a danger which may exist on a small scale, but, perhaps, often repeated in the same district."

The German Imperial Board of Health in a recent circular (July, 1892) urged the same view in the following words: "All water that can by any possibility have become polluted by excrement, urine, kitchen waste, or other foul material, should be carefully avoided. . . . Especially dangerous is water which can have received the discharges of the sick, *no matter how remotely*. In this

connection especial care must be taken that the water used in washing the garments or utensils of the sick shall not obtain entrance to a water supply, directly, or indirectly by being poured upon the surface of the soil in the vicinity of water."

Would the apathy of the public continue in regard to the pollution of our water supply if one or more cases of Asiatic cholera should by chance reach the Croton water-shed?

If cholera comes to this country in the spring or summer, as is extremely probable, scattering cases are just as apt to occur among persons using the privies which empty directly or indirectly into our drinking water, as anywhere else. Infection of the Croton water with the germs of Asiatic cholera may produce such consequences that no measures known to science could prevent a great calamity, no matter how well equipped the Board of Health might be, or how well able to control the spread of the disease from personal contact. The large volume of water in the Croton system would dilute the poison, and the natural purification which pertains to running water would greatly diminish the chances of danger to New York; but the possibilities of widespread disaster still remain.

The problem of disposing of the sewage, which confronts us on the Croton water-shed, is not a new one; it has been solved elsewhere in this country and in Europe.

The protection of the water courses is founded upon a principle of law. I quote from Waterman on Trespass, page 268, Sec. 851, under the heading "Defiling Streams":

"A person is bound so to use a stream running across his land as not to pollute or poison it by admixture with unwholesome substances. This is upon the principle that all who are entitled to the benefit of the water of a stream have a right to receive it pure and uncontaminated. Like the air, it is in a certain sense common property."

This, the Empire State, is far behind many other States,

if not in enacting, certainly in enforcing adequate laws for the protection of water courses.

To protect the waters used for domestic purposes throughout this whole State we need legislative enactment, and not only this, but what is quite as important, and without which all legislation is futile, a thorough and conscientious administration of the laws.

The property rights of the inhabitants of the water-sheds and elsewhere along the water courses must, of course, be protected, but the same laws which protect them should also supply us with the means of enforcing our natural right to an unpolluted water.

It is, in my opinion, a duty incumbent upon the medical profession to sound an alarm when any danger threatens the public health. This alarm has been sounded by the Academy of Medicine.

We know that our drinking water is polluted by human excrement, and we cannot but believe that the germs of Asiatic cholera may possibly gain access to it before many months have passed.

Why, then, longer hesitate in demanding that all sources of sewage pollution of our drinking water shall be removed at once? Why do we any longer trust to luck that the germs of disease, should they be present, may hurry through the faucet before we fill our goblet, or delay their transit until after our draught is drawn?

If we have a right to collect and store the water on the Croton water-shed and conduct it to the city for our uses, we cannot justly be denied the right to protect and preserve its purity.

A REPORT ON THE LEGAL MEASURES

TO BE TAKEN TO CORRECT EXISTING ABUSES IN THE SANITARY CONDITION OF THE CROTON WATER-SHED BY THE COMMITTEE OF THE SECTION ON PUBLIC HEALTH, LEGAL MEDICINE, AND VITAL STATISTICS, WAS READ BY DR. H. D. CHAPIN.

THE committee appointed by the Section on Public Health, and directed to report at this meeting concerning the laws now in force for the sanitary oversight of the Croton water-shed, submit the following:

They have first endeavored to ascertain what, if any, general law exists pertaining to the pollution of the water courses in this State; and, second, the laws pertaining to the pollution of the waters on the Croton water-shed. There seems to be a general opinion among those consulted that a principle of law exists forbidding the pollution of potable waters, but it has not been possible for your committee, within the short time at their disposal, to find any specific law to this effect. It seems, however, reasonable to suppose that common law should not allow the pollution of potable waters.

It has been learned that in 1885 a law was enacted (Chapter 543, Laws of 1885) giving to the State Board of Health the power to draw up rules and regulations to prevent the pollution of streams throughout the State, which required the approval of a county judge before they became operative in any county. In 1888 (Chapter 52, Laws of 1888) the law was amended, referring the approval of such rules to a county judge or a judge of the Supreme Court. We are informed that this amendment was made necessary by the refusal of county judges to approve of the execution of the regulations deemed neces-

sary by the State Board of Health. In March, 1889, the State Board of Health passed certain rules and regulations pertaining to the Croton water-shed, which were approved by Justice J. O. Dykman, of the Supreme Court. They are, in substance, as follows:

No privy or place for the deposit or storage of human excreta shall be constructed, located, or maintained within fifty feet, horizontal measurement, of the high-water mark of any lake, pond, or reservoir, or within thirty feet, horizontal measurement, of the high-water mark or precipitous bank of any spring, stream, or water course of any kind, tributary to said lakes, ponds, or reservoirs, on the entire water-shed of the Croton River, etc.

No privy vault, pit, or cesspool, or non-transportable receptacle of any kind for the reception or storage of human excreta, shall be, etc., within two hundred and fifty feet of the high-water mark of any lake, etc., or within one hundred and thirty feet of any spring, stream, or water course, etc.

Every privy which is constructed between the limits of fifty and two hundred and fifty feet, or within the limits of thirty and one hundred and thirty feet, and from which the contents are not at once removed automatically, etc., shall be arranged in such manner that all such excreta shall be received and temporarily maintained in suitable vessels or receptacles, which shall at all times be maintained in an absolutely water-tight condition, and which will admit of convenient removal to some place of ultimate disposal, as hereinafter set forth.

The directions in regard to house-slops, sink-wastes, water in which clothes or bedding have been washed or rinsed, are that they shall not be thrown or discharged into any lake, pond, or reservoir, or into any spring, stream, or water course tributary thereto, etc., or within fifty feet of any lake, etc., or within thirty feet of any spring, stream, or water course tributary to such lake, etc.

The disposal of garbage and refuse, manures, composts, and similar matters, dead animals, vegetable refuse, manu-

facturing wastes and cemeteries, also receives due consideration.

Under the heading "Penalty" we read that, in accordance with the Laws of 1885, a penalty of not less than fifty dollars nor more than one hundred dollars is hereby imposed upon any corporation, person, or persons guilty of a violation of, or non-compliance with, any of the above given mandatory rules or regulations; to be recovered under said act.

Chapter 468 of the Laws of 1890 amends the law of 1888, under which the above rules were established, as follows:

"Whenever, for the protection of a water supply from contamination, changes are required for the removal or purification of sewage, the municipality or corporation owning the water works benefited thereby shall, at its own expense, construct and maintain such system or systems of sewerage, or change thereof, and also provide such means of removal and purification of sewage, and also such works or means for sewage disposal, as shall be approved by the State Board of Health; and when the execution of any of the said regulations of the State Board of Health will occasion or require the removal of any building or buildings, the municipality or corporation owning the water works benefited thereby shall, at its own expense, remove said buildings and pay to the owner thereof all damages occasioned by such removal; and when the execution of any such regulation will injuriously affect any manufacturing or industrial enterprise which is not a public nuisance, the said municipality or corporation shall pay all damages occasioned by the enforcement thereof. And until such change of system of sewerage, etc., is made by said municipality at its own expense, there shall be no action or proceeding taken against any person or corporation for the violation of any regulation of the State Board of Health under this act; and no person nor corporation shall be considered to have violated or refused to obey any such rule or regulation, etc."

After the inspection of the water-shed by the Health Department of this city in 1891, a communication was sent by that department calling attention to "the dangers disclosed by the reports of recent inspections, by the contamination of the water supply of the city from a number of sources."

The Commissioner of Public Works, to whom the subject was referred, sent a communication to the Mayor which includes the following statement (*New York Times*, September 24th, 1891):

"As soon as the rules and regulations became effective by the approval of Judge Dykman, a little more than a month before my accession to this office, the department began and proceeded with all possible energy and diligence to issue the notices called for by the law to parties violating the rules and regulations and maintaining nuisances detrimental to the purity of the water, and to report to the State Board of Health the cases in which the notices were disregarded. This was continued until the passage of the act, Chapter 468, Laws of 1890 (June 2d), which practically and effectually renders any proceedings under the regulations of the State Board of Health nugatory."

A bill was introduced into the Assembly on the 19th of January, 1893, entitled "An Act to provide for the sanitary protection of the sources of the water supply of the City of New York by the acquisition by the Mayor, Aldermen, and Commonalty of the City of New York of such lands, tenements, and hereditaments, and the taking, acquisition, or extinguishment of such estates, rights, terms, easements, privileges, or interests therein, in the counties of Westchester, Putnam, and Dutchess, as may be necessary for such purpose, and the sanitary protection of the same when they shall have been acquired or taken."

The bill was reported favorably, with amendments, by the Committee on Affairs of Cities, and is known as Assembly Bill 233,877.

On motion the report of the committee was adopted.

DISCUSSION.

DR. E. G. JANEWAY said: We have heard the reports read here to-night, and we heard Dr. E. W. Martin's address at the last meeting of the Section on Hygiene, as well as the statement of the Board of Health showing the pollutions of the Croton water. Of this pollution there can be no doubt. Of the desirability of getting rid of this pollution, and the necessity of getting rid of it, there can be no doubt; but there can be a doubt as to what is the wisest way of getting rid of it. The Legislature and the Governor of this State are responsible for the passage of a law which did not carry with it a measure of efficiency for the protection of the Croton water-shed. This shows the harm which may come from legislation which is not conducted for the best interests of the people, but more or less for private ends. We have a right to protest against this legislation of 1890 which prevented efficient measures of protection from being carried out. We have a right, as physicians, to criticise the action of the Legislature which passed the bill to destroy the efficiency of the bill passed by the Board of Health. Typhoid fever always threatens us from polluted water, and now that, in addition, cholera threatens us, we are asked to entrust one man, the Commissioner of Public Works, with the power of buying and condemning lands. It seems to me that the wording of this law affords a loophole for tremendous risks, and for much trouble in the future. What we desire is the passage of a law that does more than simply protect the Croton water-shed. The entire State is filled with cities, villages, and towns whose water supply is in danger. There should be a broad, general law enacted to cover this entire question. There should be general principles laid down upon which such a law must be based.

The health of New York City is in danger on account of its polluted water supply; but I have no doubt that efficient measures can be taken to prevent this pollution without the necessity of condemning and purchasing land to the extent which seems to be contemplated under this new law, and without any such interference with private rights. It seems to me that the proper sanitary authorities should be entrusted with the protection of the Croton water supply, rather than the Commissioner of Public Works. The State Board of Health, as provided for

under the older law, should have jurisdiction in this matter; they are in a position to deal with the question, not only in this county but in other counties throughout the State also. Legislation on this point should provide for the appointment of a commissioner of the water-shed, if necessary, with proper assistants appointed and approved by the State Board of Health. Under the advice of a sanitary engineer, all possible sources of pollution should at once be removed or guarded against. We are asked hurriedly to give our support to a bill the possible outreachings of which we have no conception of.

DR. T. M. PRUDDEN said: There can be no doubt that the pollutions of the Croton water-shed are such as to justify grave apprehensions for the welfare of this city in the immediate future, if intelligent means be not at once taken to stop them. What these means should be is the important point. The greatest source of pollution is the sewage and waste of the towns and villages on or near the streams. If this were properly cared for the most urgent danger would be removed.

This problem of the safe disposal of the sewage and waste from small towns and villages has been so often and so satisfactorily solved that, under the advice of competent sanitary engineers, an efficient plan might be made and carried out at a comparatively small expense. It has recently been proved that filter tanks for sewage can be made at slight expense which will remove all the harmful elements. By this plan it has been shown that ninety-four per cent of the organic matter and ninety-eight per cent of the bacteria, and even, in some cases, a higher percentage of the impurities, can be removed. This is only one plan of disposal of sewage which would be applicable to the Croton water-shed. There are many others.

But any plan suited to the special conditions of the problem before us must be furnished by sanitary engineers. Just here we must not forget that familiarity with letters, with business, with law, with medicine, with accounts, or even with municipal politics, does not, in itself, make any man's opinion on such sanitary problems as this valuable. Nor does it justify him in assuming the functions of a sanitary expert or of a judge of what is necessary for the protection of the public health.

With a well-studied plan for the safe disposal of the sewage and waste of the towns and villages on the Croton water-shed, and power and means to execute it; with a

reasonably stringent law for the prevention of the pollution of the water by individuals and manufacturers; with an honest and efficient administrative corps in charge of inspection, and with the enforcement of law, there would be a comparatively limited necessity for acquirement of land by the city.

The bill before the Legislature on this subject contains one provision to which I think we should strongly object, as citizens as well as physicians, namely, the conferring on one individual of the power to take land. Such power should be in the hands of a commission of several men. It should be mandatory on this commission to acquire such land only as, in the opinion of sanitary experts and engineers, would be desirable or necessary in the carrying out of a consistent plan for the protection of the water. It would be very easy indeed for the taxpayers of this city, through the Commissioner of Public Works, to dispose of half a million dollars yearly in the purchase of real estate in the vicinity of the streams of Putnam, Westchester, and Dutchess counties, without in any essential particular improving the condition of the Croton watershed.

What this city needs then is, in my opinion: First, a consistent plan, approved by sanitary experts, for the safe disposal of town and village sewage on the water-shed, and legal and financial power to carry such plans into effect. Second, such legislation as shall forbid, under compelling penalties, individual pollution of the water, and an honest and efficient enforcement of that legislation. Third, the city needs the power and means to acquire, under proper safeguards of individual rights, such real estate as may, in the minds of competent sanitary experts and engineers, be necessary to protect the water after the sewage problem shall have been, as far as possible, solved by the more simple and effective measures which science has made known.

Dr. E. G. Janeway said: I move that the paper which has just been read by Dr. Prudden be considered as the sense of this meeting, and that it be transmitted to the Legislature and to the Governor of the State as the report of the New York Academy of Medicine.

Motion seconded.

Dr. L. Weber said: I have been very much interested in the paper and lantern illustrations presented by Dr. Cheesman. I have spent much time in that portion of

Westchester County, and know from my personal knowledge that the sewage from the condensed-milk factory, as well as that from the hotel and a number of adjoining houses, empties into the stream supplying the Croton water-shed. I think that all of us who have listened to the paper, and to the report read by Dr. Chapin, must have come to the conclusion that it is a pity that the law framed in 1888, and approved by the Supreme Court Judge, Judge Dykman, was practically abolished by the amendment passed by the Legislature of 1890. If the original law were still in effect, and if a set of officers were appointed to patrol the stream, it would be sufficient protection without the necessity of going to the great expense of buying the property in the neighborhood of the stream. It may be necessary, however, to do this in certain locations, as, for example, the ground covered by the condensed-milk factory and the hotel. I heartily indorse the suggestion that the Academy of Medicine should do all in its power to prevent the passage of the bill now before the Legislature.

C. F. CHANDLER, PH.D., said: I fully indorse the statements made by Drs. Prudden and Janeway. The bill now before the Legislature for the protection of the Croton water-shed is a very unwise one, and when I first saw it referred to in the daily papers I was afraid lest the Academy might be induced to adopt resolutions regarding it which would naturally carry with them great weight and perhaps lead to the passage of the bill. The Croton water-shed includes an area of three hundred and forty square miles, and this law gives the Commissioner of Public Works authority to spend five hundred thousand dollars yearly for three years for the purpose of acquiring possession of the real estate in that vicinity. This I regard as a very unwise scheme for the protection of the Croton water-shed. Not one-fifth the amount is necessary. I think that a very carefully devised plan should be drawn up by persons who are familiar with this subject.

I do not, however, think there is any immediate danger from the pollution of our Croton water. While I am quite in accord with the sentiment that everything possible should be done to prevent the pollution of the water, I have had occasion to study this subject for many years, and have been compelled to adopt views regarding it which are widely at variance with those popularly held. In 1872 the Water Commissioners of the city of Albany, N. Y.,

were considering the question of obtaining a proper supply of water for that place. Various propositions were made. The city was at that time very heavily taxed, and the commissioners came to the conclusion that they could not afford to go a very great distance for their water supply, which would involve the expenditure of large sums of money. As the Hudson River flows in front of the city, the commissioners were advised to pump the water from the river into their aqueduct and thus supply the city. I was sent for, and the question was put to me whether it would be safe to use the water from the Hudson River. I, of course, replied that I would not recommend such a water supply; that the river water had much drainage flowing into it and was far from appetizing. The commissioners said they could discuss the æsthetic side of the question themselves; what they wanted to know from me was whether the water supply was safe, whether it would produce disease or not. Feeling that my opinion involved much responsibility, I took a sufficient amount of time before giving it. I looked over the publications of foreign countries, wherever the subject of water supply had been studied, and was finally led to the conclusion that it was perfectly safe for them to drink the water from the Hudson River. I made such a report and the commissioners acted upon it, and the people of Albany have been drinking the Hudson River water ever since. Albany has a population of one hundred thousand, and Troy, which is a few miles further up the river, has a population of sixty thousand. In 1885 the question of increasing the water-pumping facilities of Albany came up, and in the meantime the bacillus theory having been propagated, the commissioners applied to me again as to whether my views were changed. That led, of course, to a careful consideration of the bacteriological side of the question; after going over it carefully I made a statement to the effect that nothing had occurred to change my opinion. I have naturally watched the health reports of the city of Albany very carefully during the past twenty years and compared them with those of Troy. The water supply of the city of Albany is taken from the Hudson after the river has been polluted by the sewage, etc., of Troy, and still there has been less typhoid fever in Albany than in Troy. These are facts which any one can verify, and I am led by them to the conclusion that the city of New York is not in such great danger from its

water supply as to compel the Academy of Medicine to lend its influence for the passage of a bill involving the expenditure of very large sums of money with no certainty of beneficial results.

DR. A. JACOBI said: While I did not intend to speak on this subject to-night, the remarks of Dr. Chandler have encouraged me to contradict him. He has given us certain facts, but he has given us no explanation of them. The fact, if correct, that there have been fewer cases of typhoid fever reported in Albany than in Troy, does not prove that it is safe to drink polluted water. If the health of Albany is so much better than that of Troy, there must be some good reason for it besides drinking foul water. A single fact of this kind brought before us by Dr. Chandler has no weight, because there are many instances on record where the excreta of one typhoid-fever patient emptied into a brook has resulted in a typhoid epidemic among those who afterward drank the polluted water. We do not live alone in the world, and we do not base our laws and theories upon a single fact. It is well known that when drinking water is polluted by privies, etc., typhoid epidemics often result. This is so all over the world, and if it is not so in the city of Albany there is some other good reason for their immunity. I should advise Dr. Chandler to look for an explanation of his statements.

DR. T. M. PRUDDEN said: As the result of numerous recent interviews with citizens of Albany and with some of their Water Commissioners, I have been led to believe that a considerable proportion of the population of that town are very sorry indeed that they ever accepted Dr. Chandler's report. They regard their water supply as a constant source of danger. They are very sorry that they are obliged to drink it, and have been moving heaven and earth in their endeavors to get a better one.

DR. J. WEST ROOSEVELT said: The statements made by Dr. Chandler regarding the immunity of the citizens of Albany are not so difficult to explain. The sewage from the city of Troy, which drains into the Hudson River, is at once carried off by the running water, and the total bulk of water through which it becomes distributed is so great that the danger of infection is minimized. While the Croton water-shed covers a large area, its bulk cannot be compared with that of the Hudson River, and we are in much more danger here of drinking

the concentrated germs or extract of typhoid fever, or cholera if that disease visits us next summer. The whole weight of the Academy should be thrown against the passage of the bill at present before the Legislature. There is plenty of time to frame a satisfactory bill for the protection of the Croton water-shed.

DR. S. BARUCH said: The facts adduced by Dr. Jacobi and the fact referred to by Dr. Chandler may easily be reconciled by the other scientific fact that the organic matter of sewage is rapidly oxidized by its exposure to light, dilution, and admixture with air, when flowing in the current of a river.

DR. A. H. SMITH said: I wish to state that if we expect any result from our work here to-night we must guard our remarks and discuss the question simply from a sanitary standpoint. Whatever we say as citizens, or whatever objections we make on legal or financial grounds, will avail us nothing. It is only what we say from a sanitary standpoint that will carry weight.

THE PRESIDENT said: It has been my habit to go to Albany for a number of years, and I have discussed with the citizens of that city the subject of typhoid fever. It is almost universally admitted and stated by the physicians of Albany that typhoid fever is prevalent among the well-to-do people there to an extraordinary degree; so much so that they have taken to drinking Apollinaris, boiled water, etc., carefully eschewing the Hudson River water. It is not a fact that Albany enjoys any immunity from typhoid fever.

DR. W. H. THOMSON said: I think that in the form that Dr. Janeway's motion is made, namely, that the remarks read by Dr. Prudden should be adopted as the sense of the Academy and transmitted to the Legislature, we will simply place ourselves on record—nothing more. It is not proposed to present this formally to either House or to the proper committee. It is not proposed to ask an audience of such committee. I do not think such action on our part will do any good. It strikes me that the proper way would be for this Academy to adopt such a report, and then to take formal steps to have it reach the Legislature in the ordinary way in which such petitions are brought before that body. The facts recorded should be embodied in the form of resolutions. I should therefore like to amend Dr. Janeway's motion, and recommend

that a committee of five be appointed by the chair to properly present this question to the Legislature.

DR. JANEWAY said: I accept Dr. Thomson's amendment.

DR. F. A. CASTLE said: The reference by the resolution to the possible need for purchase of land by the city to enable it to protect our water supply, warrants the suggestion that the adoption of methods already more than a thousand years old involves the possession of only so much land as would be occupied by an aqueduct that would convey the supply past all sources of contamination. In the case of Tonetta Brook this would be comprised (so Dr. Cheesman says) within a distance of three-fourths of a mile, with a clear fall of a hundred or more feet. With such an aqueduct established, it would become a matter of no concern for us what disposition the town of Brewster might make of its sewage, privy contents, and manure heaps along the former course of Tonetta Brook.

DR. C. A. LEALE said that he had often examined the Croton water at the Croton Lakes, and as it ran from the pipes in the residences of New York City, for the purpose of seeking a cause for the great mortality during midsummer from gastro-intestinal diseases, and especially during the summer of 1892, which was the severest, for the long continuance of consecutively hot days, for over twenty years. Then the poor children living in New York City suffered terribly, and the mortality was so great that in one day one hundred and forty-four children under five years of age died—one every ten minutes of the day and night.

During this time St. John's Guild had on a single day on its Floating Hospital fourteen hundred and ninety-seven sick children with their mothers, sent to it by the physicians of New York City; and at the same time their Seaside Hospital for Children, at New Dorp, had more than two hundred resident patients. Most of these children suffered from intestinal troubles, and all had contracted their diseases while at home, where they were compelled to drink the Croton water, which then, according to the chemist of the New York Health Department, contained an excess of nitrites.

During the most deadly season he had made three inspections of the Croton Lake district, driving for many miles along the borders of the lake, and had seen many

preventable sources of pollution by affluent streams that even during that dry season were pouring their filthy waters, containing the results of abundant animal and vegetable decomposition, directly into Croton Lake, and some of them within half a mile of the Croton effluent. He found within one thousand yards of the aqueduct five cows defecating and urinating directly into the lake, as they stood along the banks knee-deep in the cooling waters. Also, near the terminus of the lake, horses were pasturing, and he had seen manure spread on the ground for the purpose of improving the growth of grass. He was attracted by a fearful stench to a place within five hundred yards from the water's edge, where he found a dead horse in the last stages of decomposition, swarming with devouring maggots.

The Croton Lakes afford New York City one of the most abundant sources of a pure, potable water supply. Millions have been spent on aqueducts forty miles in length. The prevention of disease is easier than its cure, and it is our duty to do our utmost to prevent the unnecessary pollution of the water that more than two millions of human beings are compelled to drink in their efforts to sustain life; and nothing can excuse our citizens from the great responsibility to use their influence to overcome such destruction of human life.

THE PRESIDENT said: The motion made by Dr. Janeway, as amended by Dr. Thomson, that the paper read by Dr. Prudden be regarded as the sense of this meeting and be transmitted to the Legislature by a committee of five appointed by the chair, is now before you. What is your pleasure regarding this motion?

Motion seconded. Carried unanimously.

THE PRESIDENT said: I shall appoint the following gentlemen to serve on this committee: Drs. E. G. Janeway, J. W. Roosevelt, A. Jacobi, W. H. Thomson, and R. H. Derby.

DR. CHANDLER said: I should like to say a few words in reply to the remarks made by Dr. Jacobi and others. I did not rest my argument regarding the safety of the Hudson River water upon a single fact. Before giving my opinion to the Water Commissioners of Albany I read up all the literature on the subject in English, French, and German, and fully investigated the subject of the supposed origin of epidemics of typhoid fever. I found that

in most of these cases the facts would not bear scrutiny. A case of typhoid fever occurred, and then it was found that a cow had been seen in the neighborhood of the brook supplying the water; such statements are not facts. Most of the opinions at present held regarding the origin of certain diseases are based on just such facts. Many of the leading authorities are fully in accord with me on this subject. Pettenkofer, of Munich, stands in the foremost rank, and he has repudiated the idea that the typhoid-fever epidemics in that city were caused by the drinking water. In reviewing the subject he stated that he had failed to find any connection between such outbreaks and the water supply. Of course the popular idea in Albany, as elsewhere, is that typhoid fever is caused by the drinking water, and many are naturally afraid of it. I was recently informed by the president of the Board of Health of that city that I was perfectly right in my opinion regarding the harmless effect of the drinking water upon the public health. The German investigators have told us that in water charged with sewage the typhoid bacilli are destroyed in less than twenty-four hours. Their vitality is destroyed by the other germs present. There is a general tendency to accept too readily false theories regarding the propagation of certain diseases, which has a pernicious effect on the public mind. The popular idea that scarlet fever is spread by sewer gas does an immense amount of harm, because it prevents proper precautions in other directions. There are very few people who are willing to say a good word for Croton water, while it is always for somebody's interest to push the various brands of table waters. I have long felt it my duty to stand up for our Croton water supply. I think it is good water to drink, and have no fear of the deadly nitrites which the chemists in the Health Board recently found in the water. All natural waters contain nitrates; they are the final form into which the organic ingredients of the water are converted by the various fermentative processes. Traces of nitrites are not uncommon in excellent waters.

Dr. R. H. Derby said: I move that the thanks of the Academy of Medicine be extended to Dr. Cheesman for his excellent paper and interesting photographic views. Carried.

Stated Meeting, March 16th, 1893.

THE SURGERY OF GALL-STONE OBSTRUCTION.

By ROBERT ABBE, M.D.

It may well be conceded that the management of gall-stone obstruction of the biliary ducts, in the vast majority of cases, is one that interests the practitioner of medicine, and is best left in the hands of the physician rather than the surgeon. It is probable that not one per cent of patients suffering from gall-stone colic come to a condition that jeopardizes life. The malady is grave often, and tedious. It is complicated by various surgical accidents, ulceration, perforation, septic infection, exhausting fever, profound cholæmia, any and all of which may be ultimately recovered from if we choose to wait. The physician of conservative practice who intelligently watches the wonder-working of Nature, stores in his memory scores of cases of gall-stone colic where, even after repeated and profound illnesses, the patients have convalesced to perfect and enduring health. We must all acknowledge, however, that when the grave forms of obstruction ensue, our responsibilities in delay increase with each day's demonstration that surgery is prepared to cope with the evil we are watching.

The picture of the parts in trouble that presents itself to the mind of the observer at the onset of an attack is not a uniform one. Diagnosis is by no means simple, less so for the mild cases than for those more pronounced. One sees many forms of gastralgia, dyspepsia, pleurodynia, colic from colon distention, and other recurrent discomforts that closely resemble the gall-stone colic when very small stones are passed. The diagnosis rests with

an associated group of symptoms rather than physical signs—to wit: pain and constricted feeling behind the right hypochondrium rather than in the abdomen, nausea, tenderness to pressure, absence of fever and of intestinal distention. There is very rarely jaundice, and the illness is over in a few hours.

The picture of such cases comprises the obstruction of the ducts by stones only a little larger than the ducts and which are easily moved on by peristalsis. When we remember that statistics show that one in ten of those who reach middle life are known to have gall stone, we are the more ready to believe that the larger proportion of cases of brief spasmodic attacks of pain in the anterior hepatic region are due to light gall-stone attacks. Yet we may be deceived, by the absence of jaundice and a lull in the acute symptoms, into thinking the stone has passed, whereas it has only become permanently lodged in the cystic duct. In this condition a group of cases will be found showing a picture of pretty uniform type. The cystic duct becomes blocked permanently about as often as the common duct. If a stone no larger than a white bean excites inflammation enough in the duct to produce ulceration, a cicatricial stenosis ensues which prevents the stone moving either way. Other stones remaining in the gall bladder beget cholecystitis, and a copious inflammatory exudation ensues, with tumor and pain.

The fluid is found, after a while, entirely free from bile, either milky or colorless. It may form a large, recognizable tumor, which in time may be absorbed, leaving only an atrophied remnant of gall bladder encysting the incarcerated stones painlessly. But this is not usually the case. Repeated efforts are made to throw out the foreign bodies, and correspondingly persistent pain without jaundice brings the patient to the surgeon. This picture is one of the most common and has yielded many of the most satisfactory results of the recorded operations. I can best illustrate it by a case from my own books.

A young woman of thirty years came under my care

with a history of many attacks during ten years, the intervals ranging from two weeks to three months. The severity of the attack had induced morphine habit. Jaundice had sometimes supervened, but not during recent years. She had become emaciated and discouraged. The gall-bladder region was tender on palpation, but no tumor could be felt; even her corset pressure was painful.

On operation I found the gall bladder adherent to the stomach by old adhesions. It was not larger than normal, but on section contained a thin, whitish, mucous fluid and fifty-three small stones. The presence of fluid devoid of color, and, chemically, of bile also, in the gall bladder is always conclusive proof, as in this case, of the obstruction being confined to the cystic duct. My inability to pass a probe through the duct, or to feel a stone in it on palpation, was no proof that there was not a small stone there. The patient made a quick recovery, with a permanent mucous fistula from which bile never came. Six months afterward I reopened the abdomen, dissected away the entire gall bladder, tied a ligature about its duct, and found there a small stone incarcerated between two tight strictures, as if it had been lodged there for years.

The patient made an uninterrupted recovery, abandoned the morphine habit, and for four years has maintained robust health, without the least recurrence of pain.

In another case, reported elsewhere, a young married woman came under my care in a condition of grave hectic and debility, with a tumor on the right side below the ribs. This had existed six months, with vague colicky pains at first, but never a frank paroxysm. She had never been jaundiced. The mass was tender, hard, and movable. It had been diagnosed a cancer.

I opened the abdomen and found a dense, malignant-looking mass as large as one's fist and universally adherent. On cutting into it I felt that it was even more

certainly, from clinical appearance, a cancer. In its centre was a small amount of muco-pus, scarcely two drachms. No stone could be felt within it, nor on examining the ducts from the mesenteric side. She made an easy convalescence. The hectic disappeared. In six months a sinus remained, through which I removed from the remnant of the gall bladder a solitary gall stone, the size of a pecan nut, which had worked back from the cystic duct toward the easiest outlet. A few months later the entire hyperplastic tumor which so resembled a hard cancer had disappeared.

Catheterism or probing of the cystic duct, when it has not been dilated by the passage of stones into the common duct, is a matter of impossibility in the majority of cases, hence the return flow of bile through the gall-bladder fistula left after operation is often the only proof we can have of the perviousness of the cystic duct. I speak only of the class of cases where obstruction is in this duct alone, as illustrated above. These constitute a considerable part of those demanding surgical relief.

It is not to be wondered at when we consider the extraordinarily tortuous and convoluted anatomy of the canal. Few who have not made a minute dissection of it can realize that there are scores of overlapping valves in the two inches of its course. Terrier and Dally, in their admirable study of the question of possible catheterization of this canal, have been unable to report success in more than one case in four.

I show an illustration of a normal duct which I hardened in alcohol after distending with alcohol, thus maintaining the most normal relations of the parts as found in life (Fig. 1).

It seems incomprehensible that a stone should ever escape from the gall bladder, and one does not cease marvelling when he has found large and small stones in every stage of progress through this minute duct. The distensibility of the canal far exceeds that of the parturient passages. Rokitansky says that a stone even as large

as a hen's egg has been known to traverse the entire canal and be safely delivered into the bowel.

One might think that the gravest cases of gall-stone disease must fall to the class of cases where obstruction has occurred in the common duct and the flow of bile is entirely dammed back into the liver. This is by no means so! The acuteness of the inflammatory onset may menace life at the first attack when the stones have not left the gall bladder. I have little doubt that the life of the patient whose case I will now narrate was saved by prompt operation.

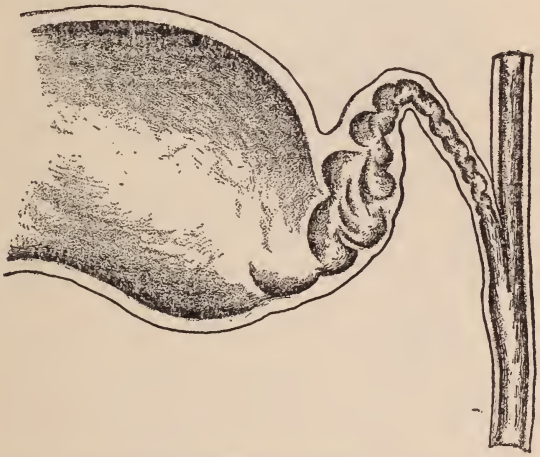

FIG. 1.—Section of normal cystic duct, showing valves.

Late in the evening of April 26th, 1892, I was called to see a lady of about forty by Dr. J. C. Warren. She had never had trouble until two days before, when severe colic, vomiting, and purging came on suddenly. For two days the gravity of the symptoms increased, and before I saw her had become extremely violent, resisting the quieting action of morphine, freely given by Dr. Warren. When I saw her she was having intense paroxysms of pain, lying with knees drawn up and moving only when forced to vomit. Her condition seemed desperate, her eyes were dark and sunken, pulse poor, and she was evi-

dently approaching collapse. It was impossible to palpate the abdomen, but a full dull area was discerned extending vertically from the liver toward the groin and ending considerably below the navel.

I operated at midnight. The gall bladder was greatly distended and enlarged downward, resting on the kidney. It was coated with a layer of new lymph, from peritonitis that had already set in, yet her temperature before operation was 100½° F. Slipping my fingers into the abdomen,

FIG. 2.—Gall-stone obstruction of cystic duct with acute distention.

I could feel only one stone at the neck of the sac. Protecting the abdomen by thin flat sponges within it, and drawing out the end of the tumor, I incised it, and evacuated a tumblerful of muco-purulent fluid, not bile, though stained with it, and seventeen gall stones (Fig. 2), eight of them large and remarkably uniform, nine small and also uniform, all spherical, the larger ones three-quarters of an inch in diameter, the smaller ones one-quarter. One of the largest ones was tightly wedged in the beginning of the cystic duct.

After evacuating the stones I closed the cut end of the bladder about a syringe and distended the cyst with Thiersch's solution, to see whether it could be emptied into the intestine by pressure and thus prove the patulency of the duct. It was impossible to force water through. I therefore sutured the cut end in the wound and established a fistula. The patient's temperature fell to normal on the following day. Bile flowed freely from the fistula, which closed spontaneously in three weeks. The patient has remained in perfect health since then, now nearly a year.

The question of the endurance of the patient to the physical strain of repeated attacks is one that may legitimately influence the surgeon in advising operation. A striking case in point may be cited here.

A lady, sixty-four years of age, had, during four months, been under Dr. E. L. Partridge's care for severe gall-stone attacks, repeated from four to seven days apart, each accompanied by clay stools, porter-like urine, and jaundice. Each time she was encouraged to hope that the last stone had passed. She was unable to leave her bed. The pain became practically continuous, but the jaundice cleared up. The gall bladder distended and could constantly be felt. It was evident a larger stone had finally blocked the cystic duct. On opening the abdomen the distended viscus could readily be lifted out of the wound. Three good-sized stones were found, as shown in Fig. 3. No suppuration nor peritonitis had been set up. Two of the stones were tightly wedged in the cystic duct, and were with care worked back into the gall bladder. The cystic duct was then found permeable to a small bougie which passed into the duodenum. I therefore sewed up the wound in the gall bladder and, after cleansing, dropped it back into the abdomen.

Recovery was uneventful, and she has enjoyed unprecedented health for the two years that have passed.

The grave condition of obstruction of the common duct presents another picture, and one in which the new lines

of surgical work have more to do. There is no point of the common duct inaccessible (though it seemed to be until very recently) from which impacted stones have not been removed. It may reasonably be said that, without operation, these cases are almost hopeless.

One must recall that anatomically the common duct is buried in the meshes of the lesser omentum and descends behind the duodenum, to which and to the pancreas head it is intimately adherent. Close to it are large veins, and beside it the portal itself. Unless distended by a stone it

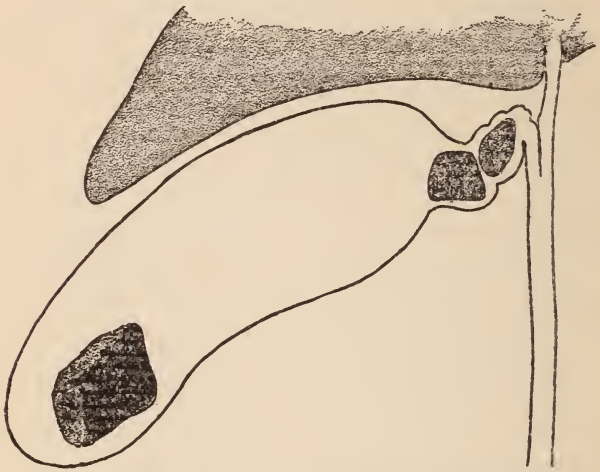

FIG. 3.—Gall stones obstructing the cystic duct.

is practically impossible to feel it. When it is distended the duct of the gall bladder is also apt to be. Such cases are illustrated by Figs. 4 to 6.

Intense cholæmia and its train of constant attendant symptoms makes the diagnosis of obstruction unquestioned. The diagnosis is not always easy, however, between cancerous and gall-stone obstructions.

The main fact, in the absence of tumor, must be the history of paroxysmal pain antedating the usual year or two that would develop a matured cancer. Chronic

jaundice without preceding gall-stone colic is most often due to malignancy.

It has been advised by surgical authorities, until very recently, that profound jaundice is in itself a serious drawback to operative work. Accumulated experience now shows that with modern thorough methods the cholæmia is not a drawback to operation.

In the following two cases the most profound jaundice had been constantly present for over two years. In one

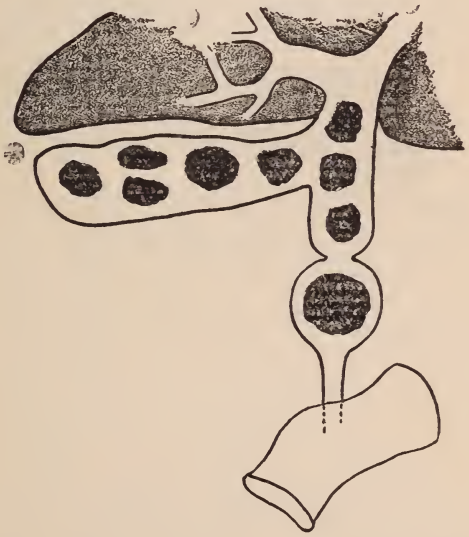

FIG. 4.—Gall stone impacted in common duct two and a half years. Removed by incision of the duct with removal of gall bladder.

the blood even had a noticeably slippery feeling from retained bile, yet both made perfect recoveries.

On April 13th, 1889, I operated on a lady, aged thirty-six, who had gone deeper and deeper in sickness after her first attack of gall-stone colic two and a half years before. She became almost a black-green, lost thirty pounds, had progressive indigestion and vomiting. She had stools entirely devoid of color, and urine like porter. Her paroxysmal pains were "terrible," and petit mal had been

recurrent daily for two months. The urine had five per cent of albumin and some hyaline casts. Palpation failed to discover a gall bladder. On doing laparotomy I found the gall bladder buried in adhesions to the stomach, omentum, and colon, but not distended. Several gall stones were removed from the incised viscus, and a large one found more than half-way down the common duct, absolutely immovable (Fig. 4). I cut the common duct open and took out the stone, then removed the damaged gall bladder, and, putting a large drain tube into the hepatic duct, sewed up the common duct with fine silk

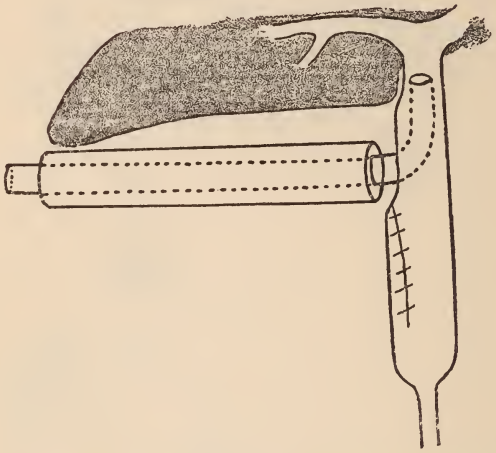

FIG. 5.—Showing method of suture and drainage after Fig. 4.

(Fig. 5). Around the drain that emptied the hepatic duct I put a larger one, reaching to the common junction, and packed in a light iodoform tampon, against which the intestines would rest and in a few hours form a solid well of lymph, precluding extravasation into the peritoneum. The patient lost all her bile through the tube for five days. The inner tube was removed on the second day, and the sinus closed in three weeks.

The patient maintains perfect health to date, having in four years had the felicity of bearing a much-hoped-for boy.

The second typical case of this group I operated upon

three months since. The patient was a married woman, referred to me by Dr. Morrison, who had been treating her more than two years for almost weekly attacks of cutting pains and biliary colic. Hypodermics of morphine had always been needed to allay suffering; jaundice and vomiting had become chronic, and the patient a wretched sufferer. There was no tumor of the gall bladder to be felt.

FIG. 6. -Large gall stone, two and a half years in the common duct.

I operated January 3d, 1893 (see Fig. 6). The gall bladder was tightly adherent to every neighboring organ. When it had been incised, dark, tarry bile flowed freely, and I removed one large and twenty small calculi. The dilatation of the ducts was wide enough to allow my index finger to ascend into the liver and down the common duct. In the latter a very large stone was found, an inch and a half below the junction, so tightly held between two strictures that it was necessary to introduce a knife

and cut the strictures from within, after which I succeeded in liberating the larger stone. The patient made a speedy recovery.

Thus I have given six cases of the gravest phases of gall-stone obstruction, all of which have made perfect recoveries. All were women.

The literature of the cases of free incision of the distended ductus communis choledochus now includes a considerable number where suture of the divided duct has resulted in immediate healing. The recoveries are so uniform that it can be regarded as scarcely more dangerous than opening and draining the gall bladder. That it is a more difficult operation to suture a small duct in a deep wound goes without saying, but the operator will find that under the pathological dilatation the walls are thickened, and when the stone is out they are free enough to be easily secured and tightened by stitches. The continuous silk suture is the best.

For the comfort of any who may be forced to leave an incised common duct unsutured, it may be quoted that during the past year Bland Sutton removed three stones from the common duct by incision, and left it open, with a tube running down to it. The peritoneum was shut off by adhesions easily generated around a small gauze tamponade. The bile discharged freely, the wound granulated and healed up in three weeks.

Keen also bored through the head of the pancreas to get at a hard knot of stones in the lower end of the common duct, and, leaving it open, drained by a tube. The wound healed kindly without suture.

The lower part of the common duct may occasionally hold the obstructing stone. This is not accessible to the knife applied from the outside of the duct, for the anatomical reason that it is hidden behind the duodenum. There is but one safe and elegant approach, if I may so speak, to it. Such an opportunity was afforded by an obstructing stone in the outlet in a case reported by Dr. C. McBurney last year, in which he incised the duodenum,

released the stone, sutured the duodenal cut, and saved the patient.

This method of searching the duodenal end of the duct I afterward attempted in a case of what proved to be malignant obstruction. As there was no stone in the duct to mark its position, it was impossible for me to identify it. It satisfied me, nevertheless, that the intestinal wound, which healed most perfectly, is an innocent addition to the surgical interference, if it be properly sutured.

Cholelithotrity, or crushing the stone in its duct, either by digital pressure only or by dressing forceps shielded with rubber tubing, has had a dozen demonstrations of its utility in soft stones since first used by Tait. Robson and Courvoisier have advocated and practised it, with only one recorded death, and that one not proven as due to the bruised duct. Marriott reported a recovery after crushing a large, hard stone through the cystic duct walls, by rubber-padded forceps, after he had vainly tried to break it up by needles. The fragments of stone have in all cases passed off spontaneously, with a slight colic, in a few hours.

The method should never be lost sight of, as it is more often available than cutting, and it may yet hold a high rank. Its present record justifies its being used where one or two moderate-sized stones are found, especially in inaccessible places. Free incision is now recognized as so safe and successful a method that any substitute for it, except for emergency, does not represent the best surgery.

One new method has entered the field to dispute for preference with the two just mentioned. That is the formation of an anastomosis between the gall bladder and the bowel, that the discharge may be directly into the bowel when the duct is obstructed irremediably.

To unite the gall bladder to the bowel by the suturing method of lateral anastomosis is a quickly accomplished and admirable procedure. The use of the vegetable or bone plates of Senn has few advocates for use in the gall bladder. Chevasse used the plates of bone to unite the gall

bladder and small intestine, but got a combined biliary and fecal fistula, which nevertheless soon healed and allowed the bile to flow into the bowel.

The preferred method of suture has been by a double row of Lembert stitches around the free incisions in the opposing viscera. It takes but fifteen minutes to do it well.

Murphy, of Chicago, has lately advocated and used successfully in three cases a simple device, the simplest that has yet been offered for speedily making anastomosis.[1] He uses two metallic buttons, shaped like mushrooms, with hollow stems, fitting one into the other. One button is inserted through a slit made into each of the opposing viscera, and sutured there; the stems remaining outside are pressed together and lock between the edges of the buttons the involved bowel and gall-bladder walls. The tight pressure causes sloughing of the included circle in a few days, and sets up adhesive union of the two edges.

Murphy used this device three times in the human subject with perfect success. The buttons were voided in due time, and the bile flowed into the bowel. Each of the cases seems to have been one where the stones might have been removed and the natural channels cleared up, without attempting to establish an anastomotic opening where the calculi could escape into the bowel, as they did after the buttons had sloughed out.

Ingenious and successful as this method is of quickly establishing a new channel for the bile into the intestine, it seems to me a retrograde step in surgical work to sacrifice the perfection of work for the sake of a brief and usually unnecessary saving of time.

Dr. Murphy operated in one case in eleven and in another in twenty-one minutes, but paid no regard to how obstructing stones in the common duct were going to be helped to escape.

The operation of anastomosis between the gall bladder and intestine was first done by Von Winiwarter in 1880;

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[1] See Medical Record, 1892, xlii., p. 665.

again by others in 1887–89, and since, in about twenty recorded cases, and has grown in favor as regards safety and utility.

It is a surgical imitation of one of Nature's methods of getting rid of stones by ulceration into the nearest viscus. It has the indorsement of every one who has tried it—Courvoisier, Terrier, Robson, and others—and seems to be unattended by risk when properly done. The fear that infectious inflammation from intestinal contamination would follow is groundless. The few autopsies that have followed at not very remote dates after operations have shown that a natural-lipped valve forms at the orifice and protects the gall bladder from regurgitation from the bowel. The greater danger appears to be that cicatricial stenosis of the opening will close it. It is a natural law that inevitably works against the surgeon in this effort. Autopsies show hundreds of scars of adherent gall bladder and bowel, but very rarely one that has kept open after the stone has ulcerated its way out. What more can we hope for the artificial opening? The only ground is, as far as I can see, in an incision of an inch and a half, which will give permanence.

Richelot, of Paris, operated by a small opening in a case of obliterated common duct, and some months later, the jaundice having recurred, autopsy showed an obliteration of the opening. On the other hand, one case (Mayo Robson's) has survived nearly three years without stenosis. In his case he joined the gall bladder and the colon at the hepatic flexure. In two other (Courvoisier's and Chavasse's) most successful cases the colon was used.

It seems to me probable that the colon may be always used with great advantage. The bile has been considered a necessary digestive secretion until of late. It has been shown that patients with chronic fistula pouring out the entire bile flow often gain weight and health. This would encourage the view that bile is excrementitious and that we can with advantage let it flow into the alimentary canal low down. The anastomosis is very easily made and

does not involve dragging the small intestine across the colon.

The subject of gall-bladder surgery was a small one when Marion Sims wrote of it first in 1878, only one case having been recorded prior to that, but now it has crept into the field of abdominal surgery with a brilliant record. Robson reports forty operations with two deaths. Tait, Thornton, Courvoisier, Terrier, Czerny, and a score of other names have added their testimony to the safety of the operation and brilliancy of the results.

The records of American surgeons are second to no others. The mortality is not more than five or six per cent. Cases of every shade of interest are being reported, until it seems now that there is almost no complication one may encounter that cannot be met by simple operative principles.

The most grave and irremediable condition is of course cancerous obstruction, and it is here that I believe the field of anastomosis of the gall bladder and colon will find its great usefulness. Except in that and in cases of absolute cicatricial stenosis of the common duct, it may be better supplanted by the judicious use of the knife, or of crushing, which in gall-stone obstruction has been so well demonstrated to be safe, no matter what part of the duct is choked.

DISCUSSION.

DR. F. P. KINNICUTT.—In considering the advisability of surgical interference in gall-stone obstruction, the possible terminations of obstruction of the cystic and common ducts respectively should be borne in mind. The possible terminations of chronic obstruction of the cystic duct are, as we know, hydrops of the gall bladder, empyema of the same, atrophy of the gall bladder even to the degree of its conversion, practically, into a fibrous band, calcification of the gall bladder, and lastly, and fortunately very rarely, an acute phlegmonous inflammation of the same. All of these terminations have come under my notice in the past two years, with the exception of the last. The diagnosis of obstruction of the cystic rather than of the common duct being made—and in this con-

nection it may be mentioned that enlargement of the gall bladder occurs as frequently from obstruction of the cystic as of the common duct, and that the distention can be even greater—and the presence of empyema being excluded, the question remains whether Nature may not be permitted to attempt a cure in the form of atrophy or calcification. It should be borne in mind that a simple hydrops, as a rule, eventually results in atrophy or calcification.

In my judgment surgical interference is indicated in cases of chronic obstruction of the cystic duct if the attacks are frequent, severe, and disabling. At the same time I believe that a fairly favorable prognosis may be given in the absence of such interference.

The terminations of chronic obstruction of the common duct are so-called catarrhal cholangitis, characterized by jaundice, chills, intermittent fever and sweats, often simulating a condition of sepsis; and suppurative cholangitis. In these forms no evidence of distention of the gall bladder frequently is appreciable. The nature of the fever in catarrhal cholangitis is susceptible of different explanations. It has been variously ascribed to irritation of the ducts (fever neurotic in character), to the formation of a ferment in the ducts, and, finally, to the presence of the bacillus coli communis. This much positively can be stated: that such symptoms may exist for many months without a trace of suppuration being found in the ducts in operation or after death. In this form recovery also can take place and after many months of suffering. Three such cases of recovery have come under my notice in the past several years. My clinical experience leads me to believe that chronic obstruction of the common duct is more dangerous to life than that of the cystic duct, and that in such cases surgical interference should be strongly advised even in absence of disablement.

DR. F. LANGE.—Dr. Abbe deserves great credit for bringing before the Academy a subject of such importance, and for treating it in such a clear and concise manner. In several respects the ideas about surgical interference in diseases of the gall bladder are still divided and varying, but this much is certain and an established fact, that surgery has already taken a strong foothold in this department of medicine, and that with increased experience and more exact technique our views will become more and more defined and precise.

I regret that Dr. Abbe has not dwelt more minutely upon the details of technique as adapted to the various kinds of gall-bladder diseases, and whether he prefers to do the operation in one or two sittings. According to my own conviction, this depends upon the nature of the case. There are cases in which the inflammation assumes a very acute and threatening character, and in which the danger of septic peritonitis is as imminent as in certain cases of perityphlitis. In these cases a delay of days, necessary for the safe adhesion of the sewed peritoneum, might be disastrous. They require immediate opening and drainage of the organ. I have had two such cases, one with suppuration in the wall of the gall bladder, the other secondary to a supravaginal amputation of an enormous fibromyoma of the uterus with suppuration in the stump. In the former case, a patient of Dr. Adler, gall stones were present; in the latter case I am inclined to assume that we had to deal with a septic metastasis. A large, fibrinous cast of the inside of the gall bladder was discharged with a large quantity of hæmorrhagic contents. Both cases ended in recovery. I think that the total extirpation of the gall bladder is not contraindicated in all cases: this procedure is generally admitted in cases of neoplasms. I have done this in one case where the gall bladder was enormously thickened, and contained a number of calculi in ulcerated depressions and several abscesses within its walls. It was clear that an organ in which such morbid changes had occurred, if left behind, must remain a source of danger. Its removal was attended with good success. This patient, and the specimen removed, were presented before the Surgical Society. About eighteen months later the patient had a severe attack of liver disease with jaundice, probably due to an obstruction of the common duct, from which she recovered. Perhaps the ligature of the cystic duct was the cause of the obstruction. I did not see nor treat the patient during that attack. In order to properly reach the gall bladder in this case I was obliged to exsect the corresponding portion of rib cartilages; this I also did in another case; and I wish to emphasize the fact that the liver is not even enlarged in all cases of chronic obstruction of the common duct, but that some, probably in consequence of interstitial shrinkage, present the organ diminished in size. The condition last referred to existed in one case in which there were numerous attacks of ob-

struction with jaundice; the gall bladder was almost entirely destroyed; one stone was found in the substance of the liver, another outside of the gall bladder between adhesions, a number of them in the fibrous remnant of the gall bladder and cystic duct, and one in the common duct near the duodenum: it was removed by direct incision. The after-treatment consisted in the employment of the iodoform tamponade, and the patient made a good recovery, although she had been in a state of chronic pyæmia, with frequent chills, as referred to by Dr. Kinnicutt, for some time. Almost a year has elapsed since the operation, and the patient is now in good health, apart from some uterine trouble.

In another case of obstruction of the common duct by stone, operated on by me at the Presbyterian Hospital about five years ago, it was possible, by repeated manipulations, to push the stone back into the cystic duct and extract it after incision. In this case I performed the so-called ideal cholecystotomy. The patient was in an advanced cholæmic condition, and died from sepsis several days after the operation. I regard the ideal cholecystotomy as contraindicated in all cases of obstruction of the common duct, because the obstruction might persevere in consequence of the mechanical lesion produced by the operative interference. The removal of stones, so long as the common duct is free, and in cases where no serious inflammatory changes have taken place, is best done "en deux temps," as recommended by Riedel, although the opening of the gall bladder need not be delayed so long. I have done the operation in one sitting with good success, but I am willing to admit that it would have been more cautious to do it in two. Several times I have seen the condition complicated with descended kidney, appearing, with the gall bladder in front of it, like an enlargement of that organ, and I once made, by mistake, a lumbar incision in such a case. An anterior incision was then made and numerous gall stones removed; the patient made a good recovery. I have met with two cases of obstruction of the common duct by malignant disease of the pancreas; one of these was complicated with gall stones. This patient bled to death by an uncontrollable capillary hæmorrhage from the surface of the gall bladder and (probably) the gall-duct system. She was cholæmic to a high degree. In the other case cholecystocolostomy was done, and about nine months later the specimen was presented before the Surgical Society.

The symptoms of these patients are ofttimes very uncertain, simulating those of other diseases. Not infrequently we get the statement that attacks of "perityphlitis" had preceded, and I have myself performed laparotomy on an elderly colleague who was sent to me for a supposed pyloric stricture. The stomach and surroundings were quite healthy, but the gall bladder contained a large number of small stones. It was fixed to the peritoneum, with the intention of opening it later, but the patient suffered from continuous uncontrollable singultus and vomiting for days after, so that the sutures were torn; the liver being small and situated high up, repeated attempts to get hold of it were futile. It was not deemed advisable to give an anæsthetic again, and further attempts at operation were abandoned. The patient did not have any further attacks of pain for a long time, and felt very well after taking the cure at Carlsbad. Since then, at long intervals, he has had mild attacks.

I have found stones of exceptional size in a patient whose gall bladder did not close after the operation. A malignant growth sprang rapidly from the bottom of the organ, apparently a large spindle-celled sarcoma, and the patient died six months later from exhaustion. In another case I was obliged to perform laparotomy for obstruction of the small intestine by a gall stone. The operation was rapidly and easily done and the stone removed, but the patient was already suffering from peritonitis at the time of the operation, and died about six hours later in collapse; the patient was a woman of advanced age and very fat. In a number of cases wherein I have attempted operation the secondary changes in the liver were so far advanced that health could not be restored. I have seen the liver studded with large abscesses, as well as a diffuse dissemination of minute abscesses, purulent in appearance, probably corresponding to the ramifications of small gall ducts. I have had under observation a gentleman who suffered from gall stones for years, with fever and slight septic symptoms, and in this case I have felt, beyond doubt, the soft crepitus due to the presence of the calculi. This man is now in perfect health, after making repeated visits to Carlsbad.

The indication for surgical help in these cases is a very important point, and in the light of future experience we will be better able to define those cases in which operations are indispensable. In others than these, operations should be avoided.

DR. G. L. PEABODY.—Nobody rejoices more than I do at the invasion of the waste places of medicine by the surgeon, who is eager to fill all the loose gaps the physician has left. This is certainly one of the most recent fields which the surgeons have invaded. I cannot help feeling, however, that they have not made quite as thorough or careful a study of the field, from a diagnostic point of view, as well as from a prognostic point of view, as to justify such a frequent resort to the knife as seems now to be recommended and acted upon. Nobody will question the brilliancy of the operations that have been detailed to us to-night by Dr. Abbe, nor the justification for those operations, as shown by the diagrams and the histories of the patients; and yet I am compelled to admit that I have seen operations performed for supposed gall-stone obstruction which were not justifiable, because we were in error about the diagnosis.

As long as the stone remains in the gall bladder it is likely to do little harm and gives rise to but few symptoms. I have often seen, on the post-mortem table, the evidences of the cure of this condition brought about by the efforts of Nature, without any assistance from the physician or surgeon. Such cures took place after the manner alluded to by Dr. Kinnicutt—that is to say, the gall bladder became atrophied; it shrivelled, and closely enveloped one or more calculi, which had been practically removed from the economy by this dense mass of fibrous tissue.

If the gall stones travel further down and get into the cystic duct, they announce their presence by severe pain, and this enables us to make a diagnosis. If we treat these cases medically, by means of opium, hot fomentations, baths, etc., the calculi are apt to pass into the common duct and ultimately reach the intestines without causing any further symptoms. Unfortunately for purposes of diagnosis, when they reach the common duct they are not so likely to announce their presence by pain as in the cystic duct. The patients sooner or later are likely to have pain, chills, intermittent fever, septic symptoms, etc., but ofttimes the jaundice is the first symptom of occlusion of the common duct. Such a painless jaundice may be caused, not by a gall stone, but by the condition of the blood, by a duodenitis accompanying gastritis, or it may be due to a variety of conditions which do not call for surgical interference. Some years ago I

had under my care a lady of advanced years who suffered from obstruction of the common duct, without pain, for more than a year. She was deeply jaundiced, the skin being of a greenish-black hue ; the bile pigment was present in the urine and totally absent from the stools. After one year, without any operative treatment, all of the symptoms gradually subsided, and she completely recovered her health.

It seems to me that in too many of these cases a surgical procedure is resorted to for the purpose of making a diagnosis. I plead guilty myself, in connection with one or two surgeons here, of having been too eager, in a few of these cases, to do something more than medical treatment permits. If we make the diagnosis and locate the calculus, then the thing to do is to resort to one of these numerous and very brilliant procedures that Drs. Abbe and Lange have detailed to us; but I should feel sorry if this Academy sanctioned laparotomy as a method of treatment on the part of everybody who is called upon to treat chronic jaundice. It should only be practised by the most deft and skilful surgeons. I have within the past year seen operations—which were intended to be exploratory laparotomies—performed by different surgeons (not members of this Academy), during which the process of exploration was carried on to such an extent as to handle roughly, and fairly maul, the abdominal contents, and to bring about a speedy termination of the patient's life. I do not oppose any of the surgical procedures that have been detailed to us, but I do say that we ought to be very careful and sure that we have the right case before attacking it by any such grave methods. I firmly believe that it is sometimes impossible to make a differential diagnosis between hypertrophic cirrhosis, with its attendant chills, fever, pain, jaundice, etc., and obstruction of the common duct. The symptoms, up to a certain point, are sometimes exactly the same. If we wait long enough we can, however, establish the diagnosis. While I have seen a good many brilliant results from this invasion of the medical field by surgeons, I have, too, seen a few results which lead me to believe that it is desirable to be very cautious.

Even as prominent a symptom of obstructive jaundice as a dilated gall bladder cannot always be made out. Indeed, sometimes it is greatly distended without projecting much below the edge of the liver. Osler narrates a

case in which it contained eighteen ounces of fluid and scarcely projected below the liver's edge.

Last week a prominent surgeon of this city operated for me upon a case of obstructive jaundice, in which the gall bladder was considerably distended, and, nevertheless, could not be felt through a thin abdominal wall, even after the patient was under the influence of ether.

My feeling is that the subject is one full of difficulties from the point of view of diagnosis; and while I welcome the surgeons, and crave their help where medicine is so powerless, I feel that a wise conservatism is all-important in cases in which the diagnosis is not clear, especially after an exploratory laparotomy has failed to reveal the nature and location of the lesion.

DR. B. F. CURTIS.—I think Dr. Peabody is very wise in sounding a word of warning as to the importance of making a correct diagnosis before operating in these cases. At the same time I think that the surgeons themselves are inclined to be very conservative about gall-bladder surgery. A striking example of this is the fact that even hydrops, when it gives no symptoms, is excluded by Czerny from operation. The selection of cases for operation is perhaps the most difficult branch of the subject, and the question can only be decided, I think, by the intensity of the symptoms, just as in many other cases wherein operation is not an absolute necessity, but is, so to speak, a palliative measure. That is to say, the condition may not be absolutely dangerous, and yet it may give its possessor such severe symptoms of one kind or another, and render life so undesirable, as to make an operation advisable.

An important addition to the technique of gall-stone surgery is Dr. Abbe's method of using a double drainage tube in certain cases. The inner tube, passed into the gall passages, leads the bile directly outside the abdomen, while the outer tube serves to drain the neighborhood of the wound.

As to whether it is preferable to perform cholecystotomy in one or two sittings, I think most practical surgeons are coming to the view that it is not necessary to make two steps in the operation. From what Dr. Lange said, however, I should think that the cases which he mentioned as requiring immediate incision are the very ones which those who were afraid of infecting the abdominal cavity would prolong to two sittings—that is, in the sep-

tic, purulent cases. I have had a single experience with a two-time operation, and then I made up my mind I would never again sew the gall bladder into the abdominal wound for subsequent incision. All of the gall stones could not be removed, and I was compelled to make another incision in the abdominal wall alongside of my fistula in order to dislodge the deepest. If the abdominal cavity is shut off by sponges the contents of the gall bladder can be kept out of it, while the surgeon is able to work within and without the gall bladder at the same time.

I was very glad to hear Dr. Lange report his case of death from cholæmia, and I think Dr. Abbe rather underestimates the danger in operating under such conditions. The danger lies in two directions—the tendency to hæmorrhage, and the fact that in cholæmia there is pronounced weakness of the heart's action. Thus in one of my cases a fatal syncope occurred on the tenth day after operation, when the local conditions, as shown by autopsy, were perfect.

I feel compelled to differ with Dr. Abbe as regards the portion of the intestines to which the gall bladder should preferably be fastened. I think the tendency of the time seems to be to fasten the gall bladder to the duodenum rather than to the colon, thus imitating Nature in the most perfect manner. Of course the former procedure is more difficult, but the surgeon can be guided by that fact. If the patient is in very poor condition, or some other good reason exists, the colon can be used. One case of death has been reported by Czerny which was accredited to the loss of bile, and it is better to try and make the fluid follow its physiological round.

DR. C. T. ADAMS.—I would like to present one of Dr. Murphy's anastomotic buttons, which Dr. Abbe referred to in his paper, and demonstrate the manner of its application. The advantages Dr. Murphy claims for this method are the following: The buttons can be easily and quickly applied; no sutures are required; the button sloughs out and is passed per anum, leaving a thin linear cicatrix, which does not contract so much as the one ordinarily left after suturing.

DR. R. ABBE.—I am glad that Dr. Adams exhibited one of Dr. Murphy's anastomotic buttons, as I have not had the opportunity of seeing them before. Now that I have seen one, my impression as to the inadvisability of

using them is stronger than ever. I should be very loath to allow a large mass like this to remain in the intestine. It might pass through, or it might become impacted; still, it would no doubt be swept out of the gall bladder and pass into the intestine. In three cases thus far reported, however, it has been successfully used. Dr. Keen wrote to me that he has used it lately in doing an anastomosis between the small intestine and the colon; the operation proved successful, so that it can be made available. My own preference, both in intestinal and gall-bladder surgery, is to use fine silk and the needle. I believe quick work can be done with it, and the work is much more secure, and it does not imperil the patient as much.

As to the removal of the entire gall bladder, this procedure is not in such favor as it was at first thought it would be. It is not a grave operation, and patients get along perfectly without the organ. It is not a difficult operation. It is not often that hæmorrhage gives trouble. The main reason for not removing the organ entirely is that it is rarely necessary. The only indications for its removal are when it is in a cancerous condition, or where it has been converted into a sinus that will not heal, and gives rise to muco-purulent discharge. Even in the latter class of cases, in nine instances out of ten, there is to be found at the bottom of it a small stone, and if that is removed the sinus will heal. After most operations on the gall bladder a fistula is preferable to the ideal operation of suturing it and dropping it back. I have done the latter operation once, and with success, because I felt sure that the ducts were clear. A fistula will heal in two or three weeks, unless there is some stone or foreign body to keep it open.

With regard to opening the gall bladder at the first operation, I think there is little reason to doubt that it is best to open it at first, notably for the reason that Dr. Curtis has given—namely, that you can handle the neck of the gall bladder, at the same time putting your finger inside to discover its exact condition, which you cannot do if you suture it and open it a few days later to get at the stone. In the vast majority of cases you cannot tell with a probe whether you are striking against a calculus or not. These stones are so soft that in order to recognize them you must feel them with the finger. Further than that, it has been shown that bile, even from a slightly inflamed

gall bladder, is innocent in the peritoneum. It soils it somewhat, but does not set up a peritonitis.

With regard to making an anastomosis with the colon instead of the small intestine, we have not much to figure upon in reaching a conclusion, but there are three cases on record in which anastomosis with the colon has proven successful, the patients living perfectly well with the bile pouring out into the colon. I see no reason why this should not be a safe procedure. It is also one of Nature's methods of getting rid of bile stones by allowing them to ulcerate through into the colon.

Stated Meeting, April 5th, 1893.

PILOCARPINE:

ITS PHYSIOLOGICAL ACTION AND THERAPEUTIC USES, WITH EXHIBITION OF SPECIMENS SHOWING CHANGE IN THE COLOR OF THE HAIR.

By D. W. PRENTISS, M.D.,
WASHINGTON, D. C.

THE drug which I have selected to bring to your attention this evening is the most important addition to the Pharmacopœia of 1880, and is one that has interested me for several years past.

In one particular its action is unique, and is confined, so far as I know, to my own experience. I refer to the action of pilocarpine in changing the color of the hair.

This action of the drug alone, however, is too limited for general interest, and the title of the paper is made to read: "Pilocarpine: Its Physiological Action and Therapeutic Uses, with Exhibition of Specimens showing Change in the Color of the Hair."

I am warned that this essay must not be too long, and therefore shall try to condense it as much as possible.

DIVISION OF THE SUBJECT.

I. *Brief Statement of Natural History.*

II. *Physiological Action.*

1. As a sudorific.
2. As a stimulant to accommodation of eye and contraction of pupil, and as stimulant to the hearing apparatus.
3. As a stimulant to the growth of the hair—action in changing color of hair.

III. *Therapeutic Uses.*

1. Most important as a sweat producer.
2. In diseases of the eye and ear.
3. Use in promoting growth of the hair.

I. *Natural History.*—(Pharmacographia), *Pilocarpus pennatifolius*, Lemaire.

A shrub growing to the height of ten feet; native of the eastern provinces of Brazil. (For plate see Bentley and Trimen, part xxxii., 1878.)

There are other species, included under the general name of jaborandi, but the *pennatifolius* is the most active and the one recognized by the United States Pharmacopœia.

The principal constituents of the leaves are :[1]

1. *Pilocarpine*, the active principle.
2. *Jaborine.*
3. *An essential oil* which shows in the pellucid spots of the leaves and belongs to the terebene series.

Pilocarpine is an amorphous, soft, amber-colored mass, discovered in 1875 by Hardy, and makes crystallizable salts with acids. Leaves contain one-half of one per cent.

Jaborine has the same chemical constituents, but differently arranged, and gives just the opposite physiological action to pilocarpine. The action of jaborine is identical with atropine.

Hence the importance that the pilocarpine be pure and free from admixture. This admixture probably explains the different results obtained by different investigators as to physiological action.

The essential oil is inert.

II. *Physiological action.*

1. As a sudorific.

Perhaps the shortest and least tiresome method of describing its physiological action will be to mention an actual case,[2] and as this is the case in which the change in

[1] Through the kindness of Merck & Co., of New York, specimens of the pure alkaloids and their salts were exhibited.

[2] Philadelphia Medical Times, July 2d, 1881.

the color of the hair occurred, it will be the more appropriate. It also illustrates the use of the drug in uræmia.

The patient, female, aged twenty-five years, was suffering from pyelonephritis with prolonged anuria. For ten weeks the amount of urine did not average two ounces daily, and this contained offensive pus. At one time—from December 16th to 23d, seven days—not a drop was passed, although the catheter was used twice daily.

Again, from January 22d to February 2d—eleven days—not a drop was excreted, the catheter still being introduced twice daily; and from January 22d to February 12th—twenty-one days—a total of but little over two ounces in all was passed. Twenty-one days of almost total *anuria.*

Previous to December 16th the hot bath and hot packing were several times resorted to, to produce diaphoresis, without effect.

Extreme uræmic symptoms had developed—no itching, but incessant vomiting, nervous restlessness, twitchings of the muscles, flushed face, severe headache, confusion of vision, and delirium.

An attempt was made to give infusion of jaborandi by the stomach, but it was rejected, as was also the solid extract in gelatin-coated pills.

Finally, December 16th, hydrochlorate of pilocarpine, one centigramme, was administered hypodermically, the patient being wrapped in a warm blanket, with bottles of hot water around her.

On two or three occasions later, when the blanket and hot bottles were omitted, there was less sweating.

From December 16th to February 22d twenty-two sweats were administered, requiring thirty-five or forty centigrammes of pilocarpine. As the patient became accustomed to the medicine it was found necessary to increase the dose to two centigrammes.

An analysis of the phenomena following the use of the drug in one of these "sweats," it seems to me, will be more graphic than a general description of the physiolo-

gical action. If it is tiresome, I crave pardon. It shall, at least, be brief.

A Pilocarpine Sweat, one Centigramme Hypodermically.—The action of the drug was carefully noted. Immediately, almost before the needle was withdrawn, the face and neck would flush bright red, and dimness of vision be noticed; then palpitation. In three minutes, slight nausea; eyes, nose, and mouth beginning to water and skin showing moisture. In seven minutes, free vomiting, profuse sweating, and salivation.

ANALYSIS OF SEPARATE SYMPTOMS.

Perspiration.—First noticed on forehead and neck; then the skin of the whole body, which had been previously dry and harsh, became moist. When sweating was fully established the water ran in little streams over all parts of the body. In the face it was with difficulty kept out of the eyes. In five minutes the hair would be saturated, and, though wiped as dry as possible, it would be again soaked in a few minutes. In odor the perspiration was offensive, and on several occasions had a distinctly urinous smell.

Salivary Glands.—In the beginning of the ordeal water flowed freely from the eyes and nose, as well as from the mouth; but when salivation was fully established the eyes and nose ceased to discharge. The saliva was viscid and tenacious, so that to clear the mouth it was necessary to use a handkerchief. Its flow was so profuse that, after thus clearing the mouth, she would not have time to get a drink to quench thirst before the mouth would again be filled. So, also, talking connectedly was prevented by the same cause. The water drank during a sweat at no time exceeded a gobletful.

Vomiting.—The vomiting continued throughout, almost without intermission, and was the most distressing symptom of the ordeal. After a spell of vomiting the patient would lie back on the pillow exhausted, hoping for a rest, but it would immediately return. The odor of the

ejecta during February was very offensive, like decayed vegetable matter.

The amount discharged in this way, which included saliva, was never less than two quarts, and often a gallon. The patient stated that she did not swallow the saliva, and insisted that the material vomited came from the stomach. This would indicate that pilocarpine also causes a fluid discharge from the gastric mucous membrane. Nausea and vomiting ceased as soon as the effect of the medicine passed off. Food was then taken and retained, although previously the stomach rejected everything.

Bowels.—Just as soon as perspiration was fully established the bowels moved—always a large action, and sometimes more than once. This action on the bowels is twofold: (1) from an increased amount of liquid in the intestines, and (2) from increase of peristaltic action. There were seldom more than two or three movements.

Action on Heart.—Pulse became rapid in a few minutes, and when the action of the drug was fully established a thumping palpitation added to the distress occasioned by the vomiting. This "thumping" could be heard at a distance of six feet, and continued with decreasing violence until the close of the paroxysm. The pulse ranged from 120 to 136, and was weak and compressible.

Eyes.—Pupils contracted to a small point. Sight became impaired at the first rush of blood to the face, and the dimness continued until it was impossible to distinguish objects beyond the foot of the bed.

As the effects wore off the exhaustion was extreme; pulse 130 and feeble. But there was a grateful sense of relief, and a disposition to sleep even before the sweating ceased. The head was no longer dizzy, pain less, stomach free from nausea, and tongue free from coating. A quiet sleep followed, lasting several hours, from which the patient awakened refreshed and hungry.

Amount of Fluid discharged during Sweat.—Of course this could only be estimated. The fluids from acts of

vomiting were caught in a basin and emptied three or four times, and each time contained not less than a quart. The blanket in which the patient was wrapped was saturated, as was also a folded sheet under the blanket. The pillow was saturated and the bolster beneath wet.

An experiment was made of saturating the blanket to as near as possible the same extent, and five pints of water were required.

Putting these together, then, we have the following calculation:

	Pints.
By vomiting and saliva	7
By saturated blanket	5
By sheet, pillow, and body clothing	2
	14

This seems almost incredible, but I believe the amount is strictly within the truth. The patient and her attendants thought the amount understated rather than exaggerated.

It is unnecessary to record further the details of this case, as I have used it in illustration of the action of pilocarpine.

Suffice it to say the patient recovered and is alive to-day.

It only remains in this case to record the remarkable change in the color of the hair which took place under the influence of the pilocarpine.

All of the patient's life (she was twenty-five years old) up to the beginning of the treatment her hair was light blonde with a yellow tinge. Twelve days after beginning the injections it was noticed to be darker, and from that time the change of color was rapid until, two months later, it was black.[1]

The hair also became coarser and thicker in its growth. The eyebrows and eyelashes, before almost invisible, came out beautifully pencilled. The hair on other portions of the body participated in the change.

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[1] Specimens were exhibited showing changes as above described.

MODUS OPERANDI OF PILOCARPINE.

Upon sweat and salivary glands:

1. Stimulates nerve centres of these glands.

2. Stimulates efferent nerves to the glands.

3. Stimulates glandular structure itself to increase the secretion.

In brief, pilocarpine is a specific diaphoretic and sialagogue by its elective action on these glands.

It increases also the secretions from the mucous membranes of the

Bronchial tubes,

Stomach,

Intestines and kidneys,

But does not increase the flow of bile.

As to the discharge from the bronchi, stomach, and intestines, it is difficult to say how much of the liquid is swallowed saliva.

There is not time in this paper to go into details of the experiments made to show the manner of the action of this drug, nor is it necessary, since they are fully discussed in the works of H. C. Wood, Lauder Brunton, Ringer, and others.

Brunton speaks of the enormous secretion from the sweat glands, and states that it amounts to from one to two pounds, and, together with the salivary secretion, often as much as eight pounds.

In the case which I have mentioned above I have estimated the amount, including the vomiting, at fourteen pounds.

I expect to have this statement challenged, and admit that it appears incredible that a girl weighing ninety or one hundred pounds could thus lose one-fifth of her weight in six hours without serious consequences. But the estimate was honestly made, and I stand by it.

Landois and Sterling give the amount of blood in the body at one-thirteenth of the body weight, and the total proportion of water to the weight of the body at 58.5 per cent. It is not to be supposed that all the water excreted

during a pilocarpine sweat comes from the blood at one time. As it is taken out of the blood it is replaced from other tissues by reabsorption.

One important point in the diaphoretic action of pilocarpine is the fact that the amount of urea excreted by the skin is greatly increased. This amount is stated to be not less than one gramme (fifteen grains) for each "sweat," which is five times more than in normal perspiration (H. C. Wood).

The importance of this fact is recognized in the usefulness of pilocarpine in uræmia, and is markedly illustrated in the above case. After three or four days of anuria, uræmic symptoms became intense and were relieved by pilocarpine.

The discharge from the bronchial mucous membrane is largely increased. This also has an important clinical significance in two ways:

1. It is this effect that makes it useful in the treatment of diphtheria, in which disease it detaches and softens the membranes by the liquid exudation beneath them, so that they can be expelled. This statement is not theoretical, but is a clinical fact, and, were it not for the depressing action of the drug on the heart, it would undoubtedly be of great value in that disease.

2. The second clinical significance of this action on the bronchial mucous membrane is that it points out a contraindication, as in cases where bronchitis or œdema of the lungs exists there is danger of drowning the patient in his own secretions.

But whatever may be the physiological action, as shown by experiments on animals, the clinical effect on the heart constitutes the main contraindication and principal danger of its use in disease. Many cases have occurred where fatal results from cardiac failure have followed its use in disease.

One notable case occurred in Dublin, Ireland, some years since, where a physician administered pilocarpine in malignant scarlatina. The patient promptly died, and

the doctor was called upon to defend an action at law for having killed the child.

A similar case came under my own observation, without, however, the unpleasant sequel.

So, also, in dropsy with weak heart, especially dropsy *due* to heart disease, care must be taken, in the use of the remedy, to administer stimulants freely at the same time.

Action on respiration follows that on the heart, dyspnœa accompanying the intense palpitation, and later there may be embarrassment from accumulation of mucus in the bronchial tubes. Temperature falls 0.9° during sweating stage (Brunton).

Another action of pilocarpine, which is mentioned only as a caution, is its effect in producing abortion. This is a frequent result where given in full dose during pregnancy. In like manner, also, it may produce premature menstruation or precipitate menorrhagia.

Bartholow mentions four cases in which labor was induced by three hypodermic injections of two centigrammes each; and out of nine cases where it was used for this purpose, it was successful in six.

GENERAL DISEASES IN WHICH PILOCARPINE IS USEFUL.

1. *Dropsies.*	Of renal origin. Of cardiac origin. Local dropsies—*e.g.*, hydrothorax. Hydropericardium and ascites.
2. *Uræmia.*	Anuria from various causes. In Bright's disease. Puerperal convulsions.
3. *Diseases of the skin.*	Especially such as are attended by dry, itching skin. Prurigo, pruritus senilis. Chronic urticaria, chronic eczema.

4. *Diphtheria.*
5. *Diabetes insipidus.*
6. *Galactagogue.*
7. *To stimulate the growth of the hair.*
8. *As an antidote to serpent venom.*

1. *In Dropsies* its value is evident from the amount of liquid excreted from the system.

In renal dropsy and in local dropsy, such as hydrothorax, its value is particularly marked.

In renal dropsy it relieves not only by taking water out of the system, thus causing its absorption, but also by the large excretion of urea it causes, thus relieving the uræmia, as in the case before quoted. Another effect, especially in acute nephritis, is to relieve congestion of the kidneys and restore them to action.

It is desirable, in administering the drug, to wrap the patient in blankets and surround him with hot bottles to increase the diaphoresis, and if he is exhausted give a stimulant.

In hydrothorax, hydropericardium, and ascites the same method is to be employed. In the former the results have been especially satisfactory. In dropsies due to heart disease special care must be taken to guard against the depressing action.

2. *Uræmia.*—Enough has already been said of its use in uræmia due to *anuria*, in the case quoted, to indicate its usefulness. In puerperal convulsions it has proved of great value.

3. *In Diseases of the Skin*, particularly those attended by itching and in which the skin is dry.

In one of the cases where it caused change of the color of the hair it was given successfully for the itching attending chronic Bright's disease, in dose of twenty to thirty drops of the fluid extract, several times daily, for a year and a half.

It caused moisture of the skin and relieved the itching.

I have seen it give prompt relief in a case of obstinate

urticaria after the usual remedies had failed. In this latter case there was a gouty rheumatic affection of the finger joints, which disappeared while under its use.

In another case of rheumatic gout of the fingers it was given successfully. So, also, in subacute and chronic rheumatism of other joints that resist ordinary treatment, pilocarpine is worth a trial on the basis of aiding the excretion of effete material from the system.

In these cases it is given by the mouth, either in form of the fluid extract or of the alkaloid, just sufficient to cause moisture of the skin and slightly increased flow of saliva, and must be continued sufficiently long to get decided excretory action.

4. *In Diphtheria.*—Dr. Guttman, of Cronstadt, was the first to insist on the value of pilocarpine in this disease, and he claimed favorable results in a large number of cases. His observation was, not that it exerted any specific action in the disease, but that it loosened and softened the false membrane by its action on the mucous membrane beneath, so that the former was easily expelled.

Following his report, Lax reported ten cases treated by pilocarpine alone, all of which recovered.

Later, Dr. E. C. Wendt[1] reported three cases which recovered under pilocarpine after other remedies had failed. On the other hand, Neumeister published twenty-eight cases thus treated with unfavorable results. Five cases were adults, twenty-three children.

The adults appeared to be influenced favorably; but with the children, thirteen out of the twenty-three died, and in eight bad effects of the drug were observed.

Neumeister's conclusion was that pilocarpine is a dangerous drug with children, and that its value in diphtheria is not sufficient to justify its use.

In a disease so fatal as diphtheria, especially the laryngeal form, any remedy which promises to avert the usual termination is welcomed with avidity.

[1] Medical Record, April 9th, 1891.

The following case illustrates the most favorable action of the remedy in this disease. It occurred in my practice shortly after Guttman published his cases.

I give only a synopsis of the case. Charles S., aged fourteen months, strong, healthy child of German parentage. Had been ailing for several days previous to July 9th, 1881, with hoarse cough. On this date I saw him, when voice was lost and he was restless.

July 10th.—Breathing loudly stridulous; voice entirely suppressed; entire fauces were covered with ashy-white membrane.

July 11th, A.M.—Worse; dyspnœa and restlessness increased. Pilocarpine, 2 milligrammes, ordered to be given every hour, milk punch *ad lib.*

July 11th, P.M.—No worse.

July 12th, A.M.—No worse. Turpeth mineral emetic given during night; dose of pilocarpine doubled.

July 12th, P.M.—Better; slept most of the day; stridulous breathing less marked: more cough, and cough looser; sweating and salivation very free; dose of pilocarpine reduced to original amount.

July 13th, A.M.—Still better; passed comfortable night, sleeping; stridulous breathing gone; false membrane has disappeared from the fauces.

July 13th, P.M.—Still improving; cough troublesome, but loose; aphonia still present.

July 15th.—Still improving; slight return of voice: some appetite.

July 18th.—Diphtheria well; slight diarrhœa.

When I first saw this case I considered it hopeless in the light of previous experience. It illustrates the most favorable action of pilocarpine in diphtheria.

I have tried it in many cases since, but without so favorable a result.

The form of diphtheria in which pilocarpine is most useful is that known as membranous croup, in which the danger is from the local obstruction to respiration rather than from septic infection and exhaustion. In these cases it favors the separation of the membrane and consequent expulsion.

5. *Diabetes Insipidus.*—In this disease it acts as a revul-

sive by attracting away from the kidneys the excessive secretion of water and directing it to the skin and salivary glands; and the habit once broken, it may not return.

One case under my observation, a German blacksmith aged about fifty years, had a sunstroke in July, which was followed in October by diabetes insipidus.

The amount of urine passed was very great, a large bucket being filled during the night, and as much more passed during the day—about four gallons in twenty-four hours.

The amount of water drunk was in proportion. The skin was dry and scaly, though in health he had always sweated freely.

After trying other remedies without relief he was ordered four grammes of the powdered leaves in infusion, the powder to be stirred up and drunk with the infusion at bedtime. At the same time the man was wrapped in hot blankets.

The following morning I found him much exhausted, unable to rise from weakness. He had sweated and salivated all night. The blankets were soaked, the mattress soaked, and there was a puddle of water on the floor under the bed. But it was the end of the polyuria; the latter never returned.

This case occurred over twenty years ago. There has been no return of the disease, and the man is still in good health for his age.

6. *As a Galactagogue.*—Its action upon the mammary gland is analogous to its action upon the salivary and sweat glands, but in a much less degree.

Many cases have been reported of its successful use when there was insufficient secretion of milk.

For this purpose it is administered internally in doses just sufficient to produce its physiological action without excessive sweating and salivation.

7. *To stimulate the Growth of the Hair.*—This it does by increasing its nutrition through the hair bulb. There are

two methods of administration for this purpose—one, the slow method, by small doses given by the stomach through a long period, in just sufficient amount to cause slight sweating and salivation; the other, the rapid method by hypodermic injections of one centigramme two or three times a week. (See reports by Schmitz, Scholler, Simon, and Pick, further on in this paper.)

8. *The Action of Pilocarpus as an Antidote to Serpent Venom.*—For the knowledge of this property of the drug we are indebted to Dr. H. C. Yarrow, of Washington, D. C., and late herpetologist to the United States National Museum.

The following letter from Dr. Yarrow explains itself:

"MY DEAR DR. PRENTISS:—In answer to your inquiry as to the influence of extract of jaborandi as an antidote to serpent venom, I will state that my experiments at the United States National Museum at Washington, D. C., upon the venom of the rattlesnake, led me to believe that jaborandi has decided antidotal value, and its active principle, pilocarpine, doubtless would be even more efficacious. These experiments were carefully made upon dogs, rabbits, and chickens, and it was conclusively shown that jaborandi, administered hypodermically after the application of the venom, protected against an otherwise lethal dose. So, also, did the administration of the drug internally immediately after the poisoning. The details of these experiments were published in *Forest and Stream* for the year 1888.

"Very sincerely yours,

"H. C. YARROW, M.D."

ACTION ON THE EYE.

Brunton divides myotics (pupil-contractors) into two classes:

1. Those which act by stimulating the endings of the

oculo-motor nerve, which supply the circular fibres of the iris—viz., pilocarpine, muscarine, nicotine.

2. Those which act directly on the circular fibres themselves—viz., eserine.

Pilocarpine contracts pupil and causes spasm of accommodation by stimulating the oculo-motor nerve endings of circular fibres of the iris, and those to the ciliary muscle.

Action upon accommodation begins after, and passes away before, that upon the iris. The effect on the eye, when applied locally, lasts about one hour and a half.

Atropine, on the contrary, dilates the pupil and paralyzes the accommodation by paralyzing nerve endings of the oculo-motor and stimulating the radiating fibres of the iris.

The importance of these physiological actions is in their relation to intraocular pressure.

I hope I may be pardoned a reference to the therapeutics of certain eye diseases in this presence, where so many are already familiar with the subject in their daily work. But all of us are not specialists, and the reference is in the line of *pilocarpine*.

Intraocular pressure is increased by the increase of fluid in the anterior chamber. As—

1. An increase in the amount secreted by the ciliary processes (aquus capsulitis).

2. Interference with its exit through the canal of Schlemm into the perichoroidal spaces (glaucoma).

3. Hyperæmia of the iris.

Intraocular pressure is diminished by opposite causes. Pilocarpine and eserine diminish intraocular pressure by drawing backward and flattening the iris, so as to allow the fluid of the anterior chamber to escape through the spaces of Fontana and the canal of Schlemm into the perichoroidal spaces.

The diseases of the eye in which pilocarpine and other myotics are useful I will merely enumerate:

DISEASES OF EYE AND EAR IN WHICH PILOCARPINE IS USEFUL.

Diseases of Eye.	Glaucoma.
	Hæmorrhage into vitreous and retina.
	Detachment of retina.
	Floating bodies in vitreous.
	Vitreous opacities.
	Commencing atrophy of optic nerve.
	White atrophy (a specific, according to Gillet de Grandmont).
	Paralysis of accommodation.
	Amblyopia from tobacco- or alcohol-poisoning (*par excellence*).
	Chronic rheumatic affections of eyeball.
	Photophobia.
	To break up adhesions of iris to lens or cornea.
Diseases of Ear.	Labyrinthine deafness.
	Aural vertigo.

I have mentioned *glaucoma* first because it is a matter of so much importance to the general practitioner that he should be able to recognize it, and at least do no harm if a case comes to him.

The majority of cases of glaucoma come first to the general practitioner, and the two symptoms—hardness of the eyeball, and rainbow colors around a light—should put him on his guard. If these symptoms are present, with dimness of vision and pain, better send the case to a specialist.

But, of all things, do not put *atropine* in the eye. This last, unfortunately, is too often done, the case being mistaken for iritis, the result of which it is unnecessary to state to this audience.

In all of these diseases the pilocarpine is useful, from one or all of its properties, in (1) reducing intraocular pressure, or (2) contracting the pupils, or (3) stimulating the muscle of accommodation.

In commencing atrophy of the optic nerve, according to Gillet de Grandmont, it is almost a specific.[1]

In amblyopia from tobacco and alcohol, also, it is credited with being a most valuable remedy.

When used only to contract the pupil and stimulate accommodation, it may be dropped into the eye in solution.

But when used in the graver internal diseases of the eye it must be pushed to its full physiological action, and had best be administered hypodermically in dose of one centigramme, this being given once daily, or once in two or three days, according to the effect.

In labyrinthine deafness and aural vertigo success has been claimed in several cases. It is administered as above. He does not explain the manner in which it acts.

ANTAGONISM BETWEEN PILOCARPINE AND ATROPINE.

This is so well known that it needs but to be mentioned.

Pilocarpine causes sweating and salivation, and increases secretion of mucous membranes; contracts pupil and stimulates accommodation; diminishes intraocular pressure.

Atropine and jaborine are the opposite in every particular.

The two are, therefore, physiological antidotes of each other.

A pilocarpine sweat can be stopped by a hypodermic injection of atropine. In experiments on frogs, where the action of the heart has been stopped by pilocarpine, it can be restored by atropine.

So, also, poisoning by belladonna, stramonium, or hyoscyamus can be, and has been, successfully treated by pilocarpine. Such cases have been mentioned by several writers.

A case is reported in the *Australia Medical Gazette* (February 15th, 1892, by Dr. Cortis) of a child, eighteen months

[1] Brunton's "Therapeutics."

old, who took four grains of sulphate of atropine and six grains of cocaine. Pilocarpine, one-seventh of a grain, was injected, and shortly after repeated. There was immediate improvement and prompt recovery.

The amount required to antagonize a given dose of atropine is stated to be four times as much as pilocarpine.

In the three following respects pilocarpine and atropine agree in action:

1. In producing frontal pain.
2. In producing pain in bladder.
3. In affecting children less than adults in proportion.

But these have no importance from a therapeutic standpoint.

We consider next the change in the color of the hair.

One case I have above referred to.

In another, a case of diphtheria in a very light-haired child, specimens taken before and after treatment show a decided change for the darker, but after a time the color appeared to fade. In this case the drug was only administered a few days.

A third case was reported in a paper read by myself before the American Medical Association, June, 1889, which I draw upon largely for the following facts:

Mrs. L., aged seventy-two years, suffering from Bright's disease, contracted kidney. Hair and eyebrows had been snow-white for twenty years. Suffered greatly from itching of the skin, due to the uræmia of the kidney disease. Skin harsh and dry. For this symptom fluid extract of jaborandi was prescribed, with the effect of relieving the itching. It was taken in doses of twenty or thirty drops several times a day, from October, 1886, to February, 1888.

During the fall of 1887 it was noticed by the nurse that the eyebrows were growing darker and that the hair of the head was darker in patches. These patches and the eyebrows continued to become darker, until, at the time of her death, they were quite black, the black tufts on the head presenting a very curious appearance among the silver-white hair surrounding them. A new growth of black hair also appeared on the scalp under the old hair.

These are the only cases thus far recorded in which pilocarpine has been supposed to change the color of the hair.

In 1879 Dr. G. Schmitz,[1] of Cologne, reported two cases in which pilocarpine stimulated the growth of the hair in alopecia. One patient, aged sixty, completely bald. Pilocarpine subcutaneously for disease of the eye. After three injections, within a fortnight the head became covered with a thick down, which grew rapidly, so that in four months no trace of the baldness was left. No mention is made of the color. In the second case, the patient, aged thirty-four, had a bald patch on top of the head the size of a playing-card. Total restoration of the hair, after two injections, in a short time.

Scholler[2] tells of similar results in animals in which alopecia had been produced by injections of bacteria.

Oscar Simon[3] relates the case of a woman aged thirty. General alopecia—head, eyebrows, eyelashes, axilla, and pudenda. In a few weeks, after twenty injections of pilocarpine, the lanugo of the whole body was restored. In other cases so treated there was no effect whatever.

Prof. Pick[4] relates the case of a man who was afflicted with alopecia areata. Two weeks after pilocarpine injections followed a fine colorless lanugo, and in twelve weeks restoration of the hair.

Ten cases of alopecia pityrodes—favorable results from same treatment. Color of hair not mentioned.

Landesberg,[5] of Philadelphia, says that in more than one hundred cases of eye disease treated by pilocarpine he observed no effect whatever upon the growth of the hair. Dose and mode of administration not mentioned.

In 1882 Julius Pohlman, of Buffalo, experimented on white rabbits by hypodermic injections of pilocarpine.

[1] Berliner klinische Wochenschrift, No. 4, 1879; Medical Bulletin, Philadelphia, 1882.

[2] Klebs' Archiv, 1879.

[3] Berliner klinische Wochenschrift, 1879.

[4] Vierteljahrsschrift für Dermat. und Syphil., 1880.

[5] Medical Bulletin, Philadelphia, 1882.

The dose used was large—one grain three times a day. No change in color was noted in pure white rabbits. In parti-colored animals—white and brown—in one a brown spot on back of head deepened and spread to a remarkable degree down the back and sides of the animal to the legs. In other individuals no change was noticed. Post-mortems on these animals showed enlarged spleen and altered suprarenal capsules.

The question of change of color of the hair is an interesting one, both from a physiological point of view and from the practical one of pathology.

The physiological aspect embraces the question of how a change of color takes place—whether in existing hairs, or produced by shedding of the hair and a new growth taking its place of a different color.

It has been doubted by good authority—Hebra and Kaposi—if the hair, after being once developed, can change except by a very gradual process. This doubt is based upon the theory that the hair has no vascular or nerve connection with the general system, and must, therefore, be independent of nervous or systemic influence. This position is, however, not tenable. The clinical evidence is positive that the hair does change color under systemic influences, sometimes gradually and sometimes suddenly.

We hear frequently of the hair turning white in a night from violent emotions, as fright, great grief, or great joy, and it has come to be a method of expressing extreme emotion to say, "It was enough to turn one's hair white."

I say it is not an uncommon thing to see mention of such cases in popular literature, but well-authenticated cases are not so often found. It is recorded in history that the hair of Marie Antoinette and Mary Queen of Scots became white suddenly from the horrors to which they were subjected.

A short time since, in conversation with an eminent microscopist and pathologist,[1] I asked how he would ex-

[1] Dr. William M. Gray, Army Medical Museum.

plain, from the basis of minute anatomy, the sudden change in color of the hair. He replied that he did not explain it; that he did not believe it happened; that the reported cases were not authenticated. He further said that, from the structure of the hair and its relation to the skin, he considered it impossible.

Duhring (third edition) is authority for the statement that Hebra and Kaposi discredit sudden canities. There is, nevertheless, no doubt of the fact that such change does sometimes occur, and, to set the matter definitely at rest, I looked up the subject in the library of the Surgeon-General's office. The following are some of the references found:

Dr. William P. Dewees,[1] of Philadelphia, reports a case of puerperal convulsions under his care. From 10 A.M. to 4 P.M. fifty ounces of blood were taken. Between the times of Dr. Dewees' visits—*not more than an hour*—the hair anterior to the coronal suture turned white. The next day it was less light, and in four or five days was nearly its natural color.

He also reports two cases of sudden blanching from fright.

Dr. Robert Fowler[2] reports the case of a girl sixteen years of age, apparently in good health, hair black; found one morning, in combing her hair, that a strip the whole length of the back hair was white, starting from a surface two inches square around the occipital protuberance. Two weeks later she had patches of ephelis over the whole body.

In the *Canada Journal of Medical Science*, 1882, page 113, is reported a case of sudden canities due to business worry. *Microscope showed a great many air vesicles both in the medullary substance and between the medullary and cortical substances.*

Dr. Graves says most authors are of the opinion that the hair, once formed, is independent of the organism,

[1] Philadelphia Medical Museum, 1807, vol. iii., p. 219.

[2] London Lancet, 1853, p. 556.

with which opinion he disagrees, instancing *plica polonica* as opposed to such a theory. He states five cases sustaining his views.[1]

In the *Boston Medical and Surgical Journal*, 1851, is reported a case of a man, thirty years old, hair scared white in a day by a grizzly bear. Was sick in a mining camp; was left alone and fell asleep. On waking found a grizzly bear standing over him.

A second case. Man of twenty-three years was gambling in California. Placed his entire savings of eleven hundred dollars on the turn of a card; was under tremendous nervous excitement while the cards were being dealt. He won. The next day his hair was perfectly white.

In the same article is the statement that the jet-black hair of the Pacific islanders does not turn gray gradually; but when it does turn it is sudden, usually the result of fright or sudden emotions.

The following cases are of change of color from white to black:

Dr. Bruley,[2] physician to the Fontainebleau, reported to the Société Médicale, Paris, in 1798, the case of a woman, sixty years old, whose hair, naturally white and transparent as glass, became jet black four days before her death (phthisis). On examination after death the bulbs of the black hairs were of immense size and engorged with dark pigment. The roots of white hairs that remained were dried up and two-thirds smaller in size.[3]

Dr. Alanson Abbé[4] mentions the case of Dr. Capen, who had become gray, but on recovery from disease his hair became quite dark.

In the *St. Louis Medical and Surgical Journal*, 1845, page 310, there is reported the case of an old man eighty-one years of age, robust and hale. Hair, from being perfectly

[1] See Dublin Quarterly Journal of Medical Science, 1847.

[2] Boston Medical and Surgical Journal, 1852, p. 406.

[3] Wilson, "Skin Diseases," p. 377.

[4] Loc. cit.

white, became black; same of the beard. This man also presented the phenomena of second sight; could read readily without glasses.

The text books on skin diseases also mention cases. Several cases of sudden canities are referred to in Ziemssen.

Brown-Séquard, in his own person, noticed one day a white hair in his beard where there was none the day previous. He pulled it out, and the next day others appeared.

This was observed repeatedly, and there was no doubt that the hair in its entire length turned white in one night. Under the microscope these white hairs showed small *air bubbles in place of the normal pigment.*

In a case of hemiplegia the hair became white on the paralyzed side. The same has been reported in cases of neuralgia.

Other anomalous cases have been noted where the hair became white in patches, and where individual hairs have been seen alternately white and black at different stages of their growth, to which condition Karsh and Landois have given the name of "ringed hair," and ascribed it to an intermittent trophic disease affecting the hair follicle.

Wilson mentions a case where the hair was gray in winter and regained its normal color in summer.

Alibert[1] and Beigel relate cases of women with blonde hair which all came out after severe fever, and when new hair grew it was black. Alibert also relates the case of a young man who lost brown hair during illness, and that which replaced it was red. In the case of an epileptic girl of idiotic type, with alternating phases of stupidity and excitement, during the stage of stupidity the hair was blonde, during excitement it was red. This change of color took place in two or three days, the change always beginning *at the end of the hairs.* Pale hairs showed an *increased number of air spaces.*

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[1] Loc. cit.

It has been frequently observed that when the hair changes color gradually, the change begins in the end and extends toward the bulb.

In conversation with an eminent ornithologist[1] on the change of color in the plumage of birds, he said: "I have lately been watching hairs in my moustache turn gray, and it always begins at the ends and extends to the roots."

Speaking on the subject with a lady (the one who furnished the specimens here shown), she mentioned the case of the physician who attended her at the seashore some years ago. The doctor's hair was long and quite gray. One day he came in to see her after having his hair cut, and she was surprised to notice that the gray hair had given place to black. Examination showed that his hair toward the ends had been white, and that nearer to the skin black. The white portion had been removed by the cutting. This phenomenon may frequently be noticed, attention having once been called to it.

The cases here collected are only a few in comparison to what might be found, but they are sufficient to prove beyond all reasonable doubt that the hair does suddenly change color under certain circumstances, and that the change takes place in existing hairs.

Analogous to changes in the color of the hair in man are the changes which occur in the lower animals. In animals and birds such changes are often periodical, as in their summer and winter coats. This occurs to a very marked degree in a great many species. Thus, the ermine in summer is dark brown, in winter pure white with only the black tip to the tail.

Among birds, the ptarmigan is white in winter and brown in summer. So with our familiar bobolink: yellow in fall, in spring black and buff.

As to the question whether, in birds and animals, this change takes place in individual feathers and hairs, or

[1] Prof. Robert Ridgeway, Smithsonian Institution.

whether all the old plumage and fur is shed by moulting, recent investigations favor the view that it is due to both. Dr. Elliott Coues ("Fur-Bearing Animals") says it may be either or both. Mr. Robert Ridgeway (Smithsonian Institution) inclines to the opinion that in birds it is accomplished by moulting.

Dr. Louis Stejneger (Smithsonian Institution) was formerly of the same opinion, but recent studies have inclined him to the belief that there is also a change in the color of existing feathers. He was led to this change of belief by a critical study of the changes in color of the black and white fly-catcher of Europe, and especially from an examination of a series of twenty-seven specimens of the narcissus fly-catcher (*Xanthophygalia narcissina*) of Japan. His studies in full appear in the "Proceedings of the United States National Museum, 1889." Dr. C. Hart Merriam, ornithologist of the Agricultural Department, in a letter dated June 12th, 1889, says: "The change from fall to spring plumage in birds is due to moult, without exception, as far as I am aware. In the case of mammals the matter is now in dispute. Probably in the majority of cases it is due in part to moult and in part to actual change in the color of existing hairs. . . . The change in color from immaturity to maturity is always due to the growth of new hairs or feathers."

That the change in birds and mammals is due, in part at least, to change of existing coats seems established. Sometimes this change is almost sudden, as where the change of season is very abrupt. In such case, of course, there would not be time for the growth of new hair or plumage.

In the golden plover (*Charadrius dominicus*) the black belly of summer changes to white in winter. While this change is taking place, individual feathers, part black and part white, may be seen.

In Bonaparte's gull, a common gull of our coast (*Larus Philadelphia*), the black of the head of summer changes to

white in winter, principally by change in color of existing feathers (Ridgeway).

Another interesting feature of this question, as bearing on the change in the color of the hair by drugs, is the influence of certain substances, administered as food, in changing the color of tissues in some of the lower orders.

In orange canaries it has come to be an established fact that, by feeding the parent birds with a certain kind of food, the active ingredient of which is cayenne pepper, the offspring will be of an orange color, and orange-colored canaries may be seen in the stores of most bird-fanciers. A food for producing orange canaries is extensively advertised by a bird-dealer in Baltimore (Bishop). It is reported that the Indians of the Amazon cause green parrots to change to yellow and red by feeding them upon the fat of a certain fish allied to the shad.[1]

Dr. Merriam, in the letter previously quoted, says: "It is well known that food affects the color in birds. Red purple finches and pine grosbeaks invariably turn yellow when caged. This is due undoubtedly to the absence of some important food element.

"In some of the zoological gardens of Europe it is the custom to send roseate spoonbills and flamingoes to Amsterdam Garden to be recolored. The particular food by which Mr. Westermann accomplishes this end is a secret, but it is believed to be a kind of shrimp or small crustacean which has a quantity of red pigment in its shell."

In the same direction are the changes of color in other tissues by particular foods. It has long been known that when pigs are fed on madder their bones become red. This fact has been taken advantage of by physiologists in studying the structure and development of bone. The phosphate of lime acts on the coloring matter of madder as a mordant. When given intermittently to a growing animal the bone presents alternate rings of red and white.

Darwin[2] mentions that pigs in Virginia eat the paint-

[1] Wallace's "Amazon." [2] "Origin of Species."

root (*Lachnanthes tinctoria*) and their bones are colored pink, and it causes the hoofs of all but the black varieties to drop off. "From facts collected by Heusinger it appears that white sheep and pigs are injured by certain plants, while dark-colored individuals escaped. . . . On asking some farmers in Virginia how it was that all their pigs were black, he was informed that the black members of a litter were selected for raising, as they only had a chance of living." Any one travelling through Virginia can verify this statement by noticing the preponderance of black pigs in that State.

Fleurens (1824) made use of madder for coloring the semicircular canals of pigeons to outline the canals more distinctly.[1]

Mr. Lucas, osteologist of the National Museum, informs me that the bones of the crow are made purple by feeding on pokeberries. Ridgeway says the bones of the Western fox squirrel are red while those of its Eastern brother are white. No cause has been assigned for the difference.

See, also, experiments by Marci Paolini in 1141.[2] He gives a very good plate of the colored skeleton of a fowl, and also of its colored egg after four months' feeding *Rubia tinctorium.* He also gives references to other authorities, the most satisfactory of which is Belchior,[3] who gives an account of feeding hogs and fowls with madder root and wheat meal. A rooster so fed died in sixteen days and showed the condition admirably. Other writers take up the subject after him in the same publication.

It is reported that among workers in cobalt and indigo the hair becomes blue; also, in artisans working with copper the hair takes a greenish hue.

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[1] See also Ferrier on "Functions of the Brain," and the writings of Vulpin, the French physiologist.

[2] "Specimen quorundam experimentorum de vi Rubiæ ad ossa ovorumque Gallinarium putamina calcariæ coloranda." No. 1 of "Miscellani Medichi," pamphlet vol. 1149.

[3] Philosophical Transactions, vol. ix., 1732.

The color of butterflies can be changed according to the food upon which the caterpillars are fed. More remarkable still, perhaps, is the change of color in the chameleon and in many insects, according to the color of the substance with which they are in contact.

The environment undoubtedly has a powerful influence upon the coloring of animals and birds. This is clearly illustrated in every museum of natural history. Specimens from arid desert regions are uniformly of a dull, faded appearance compared with those from regions of luxuriant foliage.

M. G. Pouchet,[1] in his work entitled "Mechanism of Change of Color in Fishes," says that it is due to the size of contractile color cells placed in the skin. These are under the influence of the nerves. The author found that the particular nerves controlling them (in the turbot) were nerves of the sympathetic system. By cutting the nerve supplying a particular area of the skin he had been able to retain that area unchanged in color, while the rest changed as the fish found itself on a dark or light surface.

That the eye is the means by which this change in its condition is communicated to the fish or crustacean, and that reflex action then takes place through the sympathetic nerves on the color cells of the chromatophores, is proved by the fact that when the animal experimented on is blinded no further change of color occurs when it is removed from light to dark, or the opposite.

See, also, the *Monthly Microscopical Journal*, 1871, vol. vi., M. G. Pouchet on "Study of Connection of Nerves and Chromoblasts" (principally in fishes and batrachians).

The reasons assigned by naturalists for periodical change in color of plumage or fur are twofold:

1. Sexual selection.
2. As a protection against enemies.

1. *Sexual Selection.*—The male takes on a brighter and

[1] Transactions of British Association for the Advancement of Science, 1872, p. 152.

more attractive appearance to facilitate the business of courtship and the securing of a mate.

2. *As a Protection against Enemies.*—In Arctic regions birds and mammals are usually white in winter, the color of the snow, so that they are with more difficulty found by their enemies.

Darwin supposes that originally only a few individuals took on this change, and, these being better protected, gradually, by a process of natural selection, only the white variety was left.

It is apparent from what has been said that there is very much concerning the changes of color of the hair and other appendages of the skin in man and the lower animals that is not understood. In its normal condition the color of the hair is dependent upon the hair bulb.

It is here that the melanine is secreted from the coloring matter of the blood, and from this point, as the hair grows, it permeates its cells, the intensity and shades, from black to blonde, depending principally upon the amount of the coloring matter.

In black hair the hair bulb is larger, contains a greater amount of melanine, and the hair itself is coarser and of more vigorous growth. In those cases where the hair has turned from white to black, and minute examination has been made, this has been found true.

In the case reported by Bruley, already referred to, of a woman, aged sixty, whose hair, previously white, became jet black four days before her death, the bulbs of the black hairs are described as being of immense size and engorged with dark pigment, while the roots of the white hairs that remained were dried up and two-thirds smaller in size.

So, on the other hand, in change from dark to white, the hair is finer in texture, less vigorous in growth, and the hair bulbs smaller.

The sudden change in canities, when due to violent emotions, can be explained in no other way than through the bulb. It is true that there is no direct vascular or

nerve connection between the bulb and its hair after it emerges from the skin; but it is also undoubtedly true that there is communication by osmosis between the cells of the papilla and those of the shaft and different layers of the hair.

Wilson[1] ascribes the cause of sudden whitening of hair to insufficient nutritive power of the skin; also suggests that there may generate a gaseous fluid in the hair in place of its normal constituents.

He says, further, that the fluids from the blood vessels of the skin permeate the hair, and thus change in fluids may alter color.

In all of the cases of sudden change to white, where the hair has been examined, the coloring matter has disappeared, and in its place is found an accumulation of minute air globules.

The same is true of gray hair of advancing age. How the air gets into the capillary structure has never been explained. Two possible explanations are offered:

One is that in the destruction of the coloring matter a gaseous substance may be developed. This hypothesis has received no support from observation. The other is that air finds entrance from without through the sides or end of the hair.

It is possible to suppose a condition of the bulb producing a vacuum in the hair shaft that shall cause, by suction, a drawing-in of air. This theory, which is proposed for the first time by the writer, I believe to be the true one, as explaining not only sudden canities, but also the gradual senile change to gray hair.

The view that the air finds entrance through the end of the hair is supported by the fact that the change of color begins at the extremity, and also by the observation that in all cases of change to white from dark hair the coloring matter has been found to be replaced by air vesicles.

The *erector pili* muscle has an important influence on pathological changes which take place in the hair bulb.

[1] "Lecture on the Skin."

This minute muscle has its origin in the true skin, and, passing downward, is inserted into the base of the hair bulb, so that when it contracts it lifts the hair outward and compresses its papilla.

The effect of sudden fright causes the hair to "stand on end" by contracting this muscle. Temperature has its influence with animals and birds. In cold weather (winter) the change is to white, in summer to black.

Cold, we know, contracts the skin, and thus probably causes pressure on the hair bulb. That the hair is easily influenced by external causes as well as those which come through its bulb is fully demonstrated. The mere fact that it can be so readily dyed and bleached artificially shows that the agents used for this purpose penetrate its substance.

Bleaching agents, such as chlorine, peroxide of hydrogen, and strong alkalies, act by removing the coloring matter and not by adding any *whiteness* of their own.

It remains to say a few words upon the subject of changing the color of the hair by substances taken internally, and as this paper has already exceeded the limits I had set for it, I shall be brief.

1. In the human subject the only agent, so far as I am aware, which has been charged with changing the color of the hair when taken internally, is jaborandi. Of this sufficient has already been said.

2. Cayenne pepper in changing the color of canary birds to orange.

This is a well-known fact to bird-fanciers. I tried in Washington to get a specimen to show you, but was told it was not the season for them—that they came in the autumn; also that they soon relapsed to their original color unless the Cayenne-pepper food was kept up.

3. The change of color in parrots by the Indians of the Amazon, from green to yellow or red, by feeding the fat of a certain kind of fish.[1]

4. The restoration of certain birds to their original

[1] Wallace's "Amazon."

brilliant colors at the Zoological Garden, Amsterdam, by feeding a kind of shrimp or small crustacean.

5. As analogous to the above, the effect of madder in staining the bones of pigs red, and of pokeberries coloring crows' bones purple.

It might be of interest, did time admit, to study the influence of diet and habit upon the color of hair in different nations of men—as, for instance, why the Saxons have light hair and the Gauls black hair.

It is within the bounds of possibility also that discoveries may be made in the future by which the color of the hair in the human race may be modified by judicious treatment of the parents.

Some colors of hair are not popular, especially with ladies, and it is not likely that Cayenne pepper will ever become a favorite to produce the orange hue.

Stated Meeting, April 20th, 1893.

ACQUIRED IMMUNITY FROM CERTAIN INFECTIOUS DISEASES.

A RESULT OF HEREDITY AND NATURAL SELECTION.

By J. WEST ROOSEVELT, M.D.

THE various theories which have been advanced in explanation of the protection afforded by one attack of certain infectious diseases from subsequent attacks may be summarized as follows:

1. The pathogenic micro-organism is assumed to exhaust the supply of some substance present in the bodies of unprotected people, which substance is necessary for the nourishment of the parasite, and which, once removed by it, is not reproduced by the body. This is called "the exhaustion theory."

2. The micro-organism is thought to produce within the body some substance inimical to its own existence, and this antidotal substance, once formed, is not destroyed or excreted by the body. This is called "the antidote theory."

3. It is assumed that in a successful struggle with the invading bacteria the body cells acquire an increased strength—become educated, one might say—and thus are able to destroy the enemy. This new strength they transmit to their descendants, so that the body is better able than before to repel subsequent invaders. Thus is established "tolerance" to the poison, as it is maintained.

The first theory may be regarded as untenable for several reasons, among which is the following, which seems to me to be fatal: If we accept it we must believe that Nature has provided man with a number of different sub-

stances which can have no conceivable use other than that of providing food and accommodation for pathogenic micro-organisms. These hypothetical substances are evidently not needed by the human body, since, *ex hypothesi*, bacteria remove them and they are not reproduced; yet the patient recovers entirely and is not at all the worse for his illness. It would be hard to believe that this could be the case if provision were only made for the growth and nutrition of some *one species of germ;* but when we are called upon to believe that the majority of mankind come into the world with a separate and distinct "substance" suited to the needs of the micro-organisms of small-pox, measles, scarlatina, chicken-pox, vaccinia, yellow fever, and a number of other diseases, the imagination is staggered and the reason revolts against such a preposterous idea. In a sense which will be indicated in another part of this paper, and which was not in the minds of its inventors, it may be said that there is some truth in the theory.

A certain degree of plausibility is lent to the antidotal hypothesis by the fact that, like all living things, bacteria produce by their own vital processes substances which, if sufficiently concentrated, are fatal to the producers. The great objection to it is that we know of no organic compound which is not excreted or destroyed by the body within a short time after its introduction into the system. This makes it hard to conceive that any permanent protection can be afforded by bacterial action.

Of the three explanations suggested, the third—that of acquired tolerance—is nearer to the truth than the others, yet it does not altogether satisfy the mind. While it is free from the objections which apply to the others, it seems to me weak in one important respect. It is perfectly logical to assume that the power of resistance existing in a body cell should be transmitted to its descendants, in accordance with the laws of heredity; but it is an assumption hardly warranted by experience or observation of other biological phenomena which ascribes to cells the

power of acquiring and transmitting peculiar resisting powers during a period of stress such as must exist under conditions which obtain in the infectious diseases. It seems to be improbable that such should be the result of their fight.

To me it seems that the objections to the theory vanish if we apply the law of the survival of the fittest to the problem, as well as the law of heredity. Let it be supposed that the feebler cells concerned in the struggle are for the most part killed. When recovery takes place the body will resume its original functional activity, but it will contain descendants of the cells originally strong enough to destroy the poison of the particular disease through which it has passed. Naturally the qualities of the parent cells are transmitted to their offspring. It is not so much by reason of new powers acquired by the stronger, as by reason of the destruction of the weaker cells, that immunity is afforded. This is much more in accord with Nature's methods as actually observed in the whole domain of biology. She seems to prefer to slay the weak rather than to leave them to transmit their weakness to others. The strong survive because of their strength; and in the end this is a benefit, for it tends to perpetuate and improve the species, and elevate the type which composes the majority of such species. In the long run it is far better that, relatively speaking, a few individuals should suffer and perish than that the whole number should do so at a later day. The survival of the fittest cells in the human body preserves the whole body from danger, if the theory of immunity set forth in this paper be true. Living cells then form the "substance" assumed in the "exhaustion theory" to be destroyed and never reproduced.

The evidence of its truth is largely derived from a consideration of the action of certain laws of Nature upon living organisms of complex structure—viz., animals and plants. It is therefore not to be regarded as entirely trustworthy. Reasoning by analogy is not true inductive rea-

soning; it is not based upon observed facts. Analogical evidence should be regarded by the scientist very nearly as circumstantial evidence is regarded by the lawyer. The latter cannot be considered as approaching in value the testimony of trustworthy eye-witnesses, but its importance increases with each additional demonstrated fact which tends to show that a certain allegation is *probably correct.* When a sufficient number of facts have been presented in court which, although they do not directly prove a case, make it extremely probable that certain events have occurred—as the lawyers express it, "show its probability beyond reasonable doubt"—circumstantial evidence amounts almost to proof, and has been regarded by the courts as actually proof. This should be the position of the scientist in respect to any theory which does not rest upon indisputable demonstration. The theory must explain all the known phenomena, and it must conflict with none of them. If based upon analogy, the closer and more evident the analogy the more probable the theory. As time goes on, and new observations are made in the light of more extended knowledge, each one which accords with the theory increases the probability of its truth. When all known phenomena confirm it and *no single one fails to agree with it*, it may be considered as proved. It matters not whether the hypothesis was based upon fact or fancy.

It seems to me that this hypothesis is a rational explanation of the immunity conferred by attacks of the diseases under discussion. The fact that attacks of some acute infectious disorders do not diminish the susceptibility to infection does not, as I think, conflict with it. The reasons which justify the last statement will be given in another paper. For the present it is desired only to give very briefly an outline of this theory in its relation to certain others.

If the doctrine of phagocytosis is correct—if the disease process is a direct conflict between the phagocytes and the invading germs—then surely the weaker cells

must perish, and, when the struggle is over, the body must find in the descendants of the stronger a safeguard against subsequent invasions. If it is by chemical substances produced within the body that the bacteria are overcome, the theory is perfectly reasonable. The bactericide necessarily is a product of cell activity; it is the direct or indirect action of the living tissue elements which determines the constitution of all the secretions and excretions. The blood plasma is no exception. If the latter contains as one of its normal constituents something toxic to certain forms of bacteria when present in sufficient amount, it is because some living cells produce it as part of their life work. If the presence of the germ determines the production of such a substance, not previously existing in the plasma, it is still by cell activity that it is made. If recovery depends upon speedy elimination of the bacteria or some product of their growth, or something necessary for their nourishment, again it must be accomplished by body cells. It might be that the micro-organisms of some diseases can only flourish by killing certain cells, directly or indirectly. Whatever be the method of attack or defence, the struggle is between germ and cell. The living tissue elements which are least able to withstand the stress of the conflict must succumb sooner than their stronger fellows; it matters not whether the weakness results from lack of sufficient destructive power as a phagocyte, or of sufficient power to produce an unusual quantity of some bactericide, or of adaptability to changed environment sufficient to manufacture some offensive or defensive substance different from that previously produced, or to withstand the deleterious effects of some product of bacterial life, or to remove some substance necessary for bacterial nourishment, or to do any work in the fight of any sort.

It may be objected that there is no proof that cells inherit qualities possessed by their ancestors. There is no direct proof; but to deny the fact would be equivalent to asserting that the whole is not equal to the sum of all

its parts, for the hereditary peculiarities of animals are admitted, and animals are composed of multitudes of cells. If the latter do not inherit and transmit certain peculiarities, how can the former? If spermatozoon and ovum are able (as they unquestionably are) to influence so powerfully the development of the entire body as to cause physical or mental characteristics to recur generation after generation, it is impossible to conceive that this result can be produced unless *every generation* of their descendants (the body cells) receives and transmits hereditary traits.

It seems equally inconceivable that the law of the survival of the fittest, which is of universal application throughout the whole animal and vegetable world to each individual animal or vegetable of every species, should fail to apply to every cell forming part of these individuals. No reasonable explanation can be (or, at all events, has been) adduced of the existence of such an anomaly in Nature.

It is a much more plausible supposition that qualities already possessed congenitally by the cells should be transmitted to descendants than that those acquired in a short, fierce struggle should be transmitted. Indeed, the possibility of the transmission of acquired traits is denied by some biologists. Of the congenital traits there is no doubt.

Of this theory it may be said, at least, that it is very fascinating. Is it not also suggestive and plausible? Is it not worthy of consideration as a working hypothesis, if nothing more? In another paper I shall discuss in detail the application of it to a number of diseases, and also to the protective effects of inoculations with attenuated virus and with vaccine.

NOTES OF A CASE OF CANCER OF THE UTERUS.

REMARKS ON DIAGNOSIS AND TREATMENT.

By DANIEL LEWIS, M.D.

In February, 1892, Dr. P. M. Barclay, of Newburgh, referred a patient to me with the following history:

Two months before, she was taken in labor, being then forty-five years of age and not having been pregnant in nearly twenty years. The cervix was found to be hard and entirely undilatable. No progress having been made in twenty-four hours, the cervix was freely divided through the anterior lip, when delivery of a dead child was easily effected by forceps. The patient made a good recovery, and no serious trouble was suspected until it was noticed that a free watery discharge was constant and profuse. I was then consulted for a suspected cancer, and the following condition was revealed by examination:

The discharge was decidedly urinous. As soon as the index finger entered the vagina it came in contact with the mucous lining of the bladder. Near the fundus the ragged, cut edges of the incised and indurated cervix were continuous with the borders of an enormous vesico-vaginal fistula, and the impression (which may have been erroneous) was that when the knife left the cervix it had penetrated the bladder and opened it to within an inch and a half of the meatus urinarius.

The body of the uterus was somewhat enlarged, but there had been no hæmorrhage and very little pain. A small portion of the cervix was removed, examined microscopically by Dr. George T. Elliot, and proved to be carcinoma.

Dr. Hanks saw the case in consultation, and removed

the uterus per vaginam March 20th, 1892. The involved edges of the bladder were, of course, removed, which left the fistula at the fundus about two inches wide. The patient suffered severely from shock, but otherwise made a good recovery, the highest temperature being 101° on the third day.

April 11th (twenty-two days after the hysterectomy) I made the first attempt to close the fistula from below upward, as far as the edges could be readily approximated. About two inches were thus secured. Silver-wire sutures were employed.

Three weeks later the remaining portion was closed, horsehair sutures being used, and they retained the parts in perfect apposition until the eighth day, but union did not occur and the patient was sent home to recuperate her general health, which we believed to be necessary to secure success.

In the autumn of 1892 additional union was secured, at which silkworm gut was used for sutures. The broad opening at the fundus was finally closed February 28th, 1893, by a transverse line of double horsehair sutures, after spending a few days in stretching the edges of the fistula, at Dr. R. B. Talbot's suggestion, a device which I found very useful.

The condition one year after treatment began was one of perfect freedom from any evidence of recurring cancerous disease, the fistula perfectly closed, and the general health better than for several preceding years. It should be stated that the patient's mother died at an advanced age of cancer of the uterus.

The only comment I have to make as to the details of the various operations in this case is regarding horsehair as a material for sutures. For many years I have employed it in breast amputations and in all operations about the face, in fact in all suturing of the skin. It never absorbs anything, is as impervious as glass, and can be rendered perfectly aseptic by careful washing in warm water. When thoroughly cleansed it never causes sup-

puration, can be left in position almost indefinitely, as it is never absorbed. It was upon my experience in other operations that I decided to try it in the operation on the fistula, and in comparison with the silver wire and silkworm gut it proved to be superior in many respects. Care must be used in selecting strong black hair. Some dealers in surgical supplies now keep it, cleaned and ready for use.

The history of this case emphasizes in a marked degree a few points regarding the diagnosis and treatment of uterine cancer.

1. The responsibility for a sufficiently early diagnosis to insure successful treatment belongs to the family physician. In all the list of cancerous diseases the earliest possible diagnosis, and prompt measures for the removal of the morbid growth, are essential, but in cancer of the uterus any mistake or hesitation is most deplorable. The fortunate part of my patient's case was the labor, which first revealed the induration of the cervix. It must be confessed, however, that the inconvenience resulting from the fistula was the cause of her seeking relief. The duty of the family physician is to be on the alert for this disease, and call for expert counsel if there is the slightest ground for suspicion of cancer. Until both physician and patient learn this lesson the mortality from uterine cancer will remain what it has always been, higher than from the disease in any other organ.

2. Inasmuch as the failure to make a diagnosis of the disease in the curable stage is usually due to an imperfect physical examination or to the entire neglect of such examination, I wish to call particular attention to the fact that between forty and fifty years of age is the period when a vast majority of cases develop. Fifteen out of a total of twenty-one cases in my own practice occurred between those years, while only two were past sixty and only one under forty. Whenever, then, a patient complains of *any symptoms of ulcerative disease during this period,* cancer should be suspected and an examination

made with that idea in view. A trained sense of touch is often sufficient to make the diagnosis, especially if the disease affects the portio vaginalis. When the body of the uterus is the seat of the disease, the degree of enlargement and immobility of the organ can be more surely determined by the use of an anæsthetic. Subjective symptoms are not of much value at this period in arriving at a diagnosis. Pain may be present or it may be entirely wanting. A decidedly fœtid discharge may indicate only a fibroid near the cervical canal, and a pretty constant hæmorrhage may depend upon the same cause. I once had a patient who complained of nothing but a slight pain in the back, in whom had developed cancer involving the entire cervix and the vagina in every direction, even to the external orifice.

Neither should the presence or absence of a history of heredity be considered of any importance. I say this advisedly, even though Sir James Paget is still claiming that most cases have an inherited tendency to cancer. In five hundred and fifty cases, of all varieties of the disease, in which this question has been carefully studied by myself, less than two per cent gave the slightest family history as a possible factor in the etiology. In the case that we are now discussing, the fact that the patient's mother died of cancer of the uterus at the age of seventy simply exemplifies the fact that cancer is often a disease of senile degeneration, of which the familiar epithelioma of the face in old people is a striking example.

Dr. John Williams, in his recent work, after a full discussion of the part which old lacerations play in the development of cancer of the cervix, says: "It appears to me that the facts at present known tend to show that lacerations play no part in the causation of cancer of the uterus." It is not my purpose to enter into the discussion of this question further than to state my firm conviction that any long-continued irritation, be it in the cervix or elsewhere, is the most potent of all factors in the causation of cancer. An old laceration is frequently

such a source of irritation, and when we perform Emmet's operation for repair in a patient past forty years of age we are very often removing a pre-cancerous disease, and such treatment may be of untold advantage to the patient.

3. A notable exception to the value of subjective symptoms in making a diagnosis is the appearance of a hæmorrhage (however slight) in patients who have long since passed the menopause. It is almost positive evidence of cancer when a woman of sixty complains of even a slight and intermittent metrorrhagia. Within the past week a lady of robust health, age sixty-one, came under my observation, in whom such hæmorrhage was practically the only symptom. The curette was used, the microscope revealed evidence of cancer of the body, and examination of the uterus, after removal, verified the diagnosis.

4. This reference to the microscope leads me to say a word regarding its value in these cases. In the examination just mentioned it told the truth. We should, however, never be dissuaded from removing a suspicious growth because we may have had a section examined which did not show the microscopical characters of carcinoma, sarcoma, or epithelioma. Mr. Butlin corroborates these views regarding the value of the microscope in the following words:

"It is not always easy to classify a tumor, even though its origin and microscopic structure have been carefully observed. The difference in structure between some adenomas and some carcinomas, between some fibromas or chondromas and some sarcomas, and between the products of some inflammations and some sarcomas, is so very minute, if, indeed, there is a difference, that it is impossible to discover it."

I do not wish to be understood as discrediting, in any degree, the results or value of microscopical examinations in cancer cases, but to emphasize the fact that the practitioner must depend upon the clinical features, history, and course of the disease chiefly, especially if the

early diagnosis of the disease is to be reached and the chances of cure thereby rendered most certain.

I have recently had an interesting experience with a patient for whom we had removed the right breast and axillary contents for carcinoma, and, subsequently, six recurrent growths in or near the cicatrix on the chest. At the last operation a small exostosis of the fifth rib was removed (a mistaken procedure, I now believe) and there was great difficulty in healing this wound. After three months it was decided to remove this stubborn sore, and it was handed to the microscopist, who reported it to be "simple cicatricial tissue." As there was no appearance of any other disease, the axilla having remained free since the first operation three years before, and in view of the fact that the patient's general condition was good, we at once decided that the prognosis was favorable. The sequel proved all our deductions to be incorrect, for in three months there was a large tumor of the spleen, and recurrence also at the very point where this "simple cicatricial tissue" had been found by the microscopist.

Early last year a patient consulted me for a growth in the cervical canal which the sense of touch told me was cancer, and enlargement of the body, with frequent hæmorrhage, also favored this view. A good specimen was secured, and a most competent microscopist reported that it was non-malignant. Acting upon that information, I decided upon curetting, after which an indurated base on the posterior uterine wall was found which was almost unmistakably cancerous. Subsequent history has verified that opinion, for the woman is now in the last stages of the disease. Her opportunity for a successful hysterectomy was lost through our reliance upon the microscope.

A few remarks regarding treatment are suggested by the cases to which reference has been made in this paper.

I have said elsewhere that all cases of cancer of the uterus of which I know the history from personal observation had died of the disease, except those now under treatment. The case now reported promises to break

this dismal record of mortality. Partial operation, so-called, must be relegated to the list of palliative measures only, which are of vast importance, however, if the family physician is going to continue to neglect the duty of making a diagnosis in season and insisting upon prompt surgical interference. My observation, although limited, compels me to differ from Dr. Williams' opinion again that a shell of the uterus left after a partial operation does not favor a recurrence of the disease. I would just as soon try to convince myself that the axillary glands are not dangerous when left *in situ* in breast cases.

Total extirpation is destined to be the operation for cancer of the uterus, whether the disease is cervical or corporeal, and if done in time will permanently cure a certain portion of cases.

Fortunately, increasing familiarity with the operation is decidedly reducing the enormous mortality which Schröder's early reports revealed, and hysterectomy for cancer has already become a legitimate procedure in properly selected cases. If any tissues adjacent to the uterus are already involved it should not be attempted.

DISCUSSION.

DR. H. T. HANKS.—The subject presented in Dr. Lewis' paper is one of great interest. Cancer of the uterus is receiving much attention to-day, and we have arrived at a time when the operation of hysterectomy can be performed for its relief, with a very small percentage of deaths resulting. There are a number of men in this city who are thoroughly skilled in this field of surgery; at this moment the names of at least ten men occur to me, members of the New York Academy of Medicine, who, I believe, can do a hysterectomy as well as Martin. It is a common operation. I saw Dr. Kelly, of Baltimore, do two hysterectomies in one day. It is a great satisfaction to know that we are doing this work as well as it can be done in the world. Of course there are many men throughout the country who do the operation very poorly. They take a course at one of the post-graduate schools, see a few operations at Bellevue, Roose-

velt, or the Woman's Hospital, and then go home and try their hand at it. I know how difficult I found my first vaginal hysterectomy, and how easy it seems now. I distinctly remember how hard it was for me to do my first suprapubic hysterectomy. As our experience with these cases grows, the operation becomes easier. If the specialist sees these cases early enough the operation can be safely done. Following vaginal hysterectomy there should not be over two per cent mortality, nor over four per cent following the suprapubic operation. Personally I have never lost a patient from this operation, although one died fourteen days after from intestinal obstruction. The case referred to by Dr. Lewis in his paper is a very interesting one. I remember it well, and feel exceedingly gratified with the result.

One point in regard to the contact of urine with the wound left after a vaginal hysterectomy. If the urine is healthy it will do no harm. Dr. Emmet long ago referred to the aseptic quality of healthy urine.

We must tell our friends in the schools where we teach that when they meet with a case presenting symptoms suspicious of cancer of the uterus, they should not delay in seeking advice from one who is better qualified than themselves to judge of these cases. A woman who has passed the menopause for two years or longer, and then has a hæmorrhage, should not be merely treated with ergot or hydrastis and allowed to go without a careful investigation. Another point is that one microscopical examination of the scrapings of the endometrium is not sufficient; two are not always sufficient. I remember one case, a woman forty-five years of age, who presented all the symptoms of malignant growth of the uterus. I performed a vaginal hysterectomy, and the patient has been perfectly well ever since. In that case the microscope failed to show any malignant growth before the operation. It simply showed a deep inflammation of the endometrium.

The question of heredity is an interesting one in connection with this subject. I remember a long conversation I had with Dr. Snow, of the London Cancer Hospital, and his statements practically coincide with my experience and with the statements made by Dr. Lewis to-night. Dr. Snow said "it is a popular idea with women that if their mothers or grandmothers or aunts did not have cancer they could not have it, and *vice*

versa." He further said that "there is a vastly larger proportion of women in London who have cancer of the breast or of the uterus, and who give a history of having been disappointed in financial, social, or family affairs, etc., than of those who suffer from the disease and give a history of heredity."

As regards diagnosis. To-day in New York we do not get in one-half of our cases the typical symptoms of cancer, because the trouble is discovered much earlier than formerly, before fatty degeneration and the fœtid discharge comes on, and before the patients become emaciated and show the characteristic color of those suffering from advanced malignant disease. Consequently we do not get the symptoms included in the old and classical definition of cancer of the uterus.

I again wish to emphasize the importance of early operative procedure in these cases. On one patient, who is now alive and well, I did the high operation eight years ago, and on another about seven years ago. Last August —and it was a fearfully hot month—I performed the operation of vaginal hysterectomy six times; of these cases one died of obstruction of the bowels from adhesions, on the fourteenth day; another had a recurrence in four months. The other four are doing well.

Respecting sutures, I have used horsehair, catgut, silkworm gut, silver wire, and silk. All are equally satisfactory if aseptic; all are bad when septic. Catgut and silkworm gut are good enough for any work when well prepared. But the wise surgeon will use in each case the suture or ligature best suited to the tissue which he is suturing or ligating.

Stated Meeting, May 4th, 1893.[1]

DISCUSSION ON THE APPLICATION OF SYMPHYSIOTOMY AT THIS ERA.

THE discussion was opened by DR. J. E. KELLY, who said: When speaking on the subject of symphysiotomy, my object is to remind my audience of some essential points in the anatomy of the subject and to indicate the practical inferences that may be drawn therefrom.

The object of symphysiotomy, as is well known, is to correct the disproportion which occasionally occurs between the fœtus, particularly its head, and the maternal pelvis. The pelvic girdle, or ring, consists of three bones —the two ossa innominata and the sacrum—that are bound together by ligaments of sufficient strength to resist the great forces transmitted between the trunk and the lower extremities. The ossa innominata meet anteriorly to form the pubic articulation, or the symphysis pubis, but they are separated posteriorly by the sacrum, which forms about a third of the pelvic ring, and with which the innominata form the sacro-iliac synchondroses. Symphysiotomy destroys the continuity of the ring, being the division of the pubic articulation. This portion of the ring is selected for operation because it is the most accessible and most easily repaired. The three articulations are similar in physiological character, though mechanically they differ materially. They are what is termed fibro-synovial articulations, owing to the component bones being joined by a plate of intervening fibro-cartilage in which is situated a synovial space. This cartilage is adherent to the opposed surfaces of the bones; but, owing to the elasticity of the fibro-cartilage, much mobility would be possible were it not for strong ligaments placed upon the external aspects of the three articulations. If one of those articulations be divided, the fibro-cartilage in the others can be torn asunder, or from the bones, by the application, from the inner aspect of the ring, or gir-

[1] In charge of Section on Obstetrics and Gynæcology.

dle, of comparatively moderate force. In symphysiotomy one articulation, the anterior, is divided; and by drawing asunder of the innominata the fibro-cartilages in the other joints, the sacro-iliac synchondroses, which are protected internally by weak and easily torn ligaments, are extended on one or both sides, generally only the less resisting one being torn.

The important ligaments that support the symphysis are on the anterior surface and easily divided, with one exception, the infrapubic, which is a triangular body filling the angle below the symphysis and lying between the descending rami of the two pubic bones. It is of great strength, and if intact, when the symphysis is divided, it acts as a hinge or pivot upon which the ossa innominata revolve vertically, with the consequent approximation of the ischial tuberosities and the necessary constriction of the outlet.

The division of this ligament is regarded as one of the most dangerous procedures in the operation, and some authorities thoughtlessly counsel its omission. If such incomplete operations should result successfully, it can be due only to the unnoticed laceration of the structure during the subsequent stages of delivery. The disinclination to deal with the ligament is attributable to an erroneous idea of its critical proximity to the dorsal vessels of the clitoris and the urethra. A space of nearly half an inch lies between the inferior margin of the ligament and the nearest structure, the dorsal vein. The principal disadvantage of dividing this vessel is due to its communication with the valveless venous plexuses within the pelvis and the consequent freedom with which it bleeds.

Another point to be remembered with regard to the symphysis is the occasional irregularity of the opposed bony surfaces, which are usually smooth and separated by the intervening cartilage. In some instances the posterior margins are in close and firm contact, or one margin may even overlap the other. Again, we find the surfaces thrown into ridges, or rounded tubercles, that fit into corresponding depressions on the opposite bone. In other, and fortunately rare, instances ankylosis occurs. This condition corresponds to the ossification which takes place between the opposite portions of the sternum; and when we remember that the union is a matter of development, its more frequent presence might reasonably be expected. All these possible conditions render it more

expedient, mechanically, to approach the symphysis from before than from behind, as is recommended by some authorities. The former method is indicated by Nature, as, normally, the fibro-cartilage is wedge-shaped in both directions, being wider in front and superiorly than behind and inferiorly.

In this region there are four essential muscles which it is well to remember. These muscles proceed from the crest of the pubes close to the median line, and are the pyramidalis and the rectus abdominis on each side. They form a muscular structure which must be divided before the posterior surface of the pubes is reached. The triangular space which is indicated in the diagram, No. 1, is bounded anteriorly by the recti and pyramidalis muscles, behind by the fascia transversalis, and inferiorly by the pubes. The posterior wall, formed by the fascia transversalis, of this important space serves different purposes in symphysiotomy and in suprapubic cystotomy. In the former it is preserved intact in order to protect the peritoneum and the bladder, while in the latter it must be divided and serves as a guide to the viscus. The vessels and nerves in this vicinity are few and of little surgical importance, as the external pudics are small, and, by pushing back the transversalis fascia, those lying behind the bones are removed from the seat of operation, while the dorsal vessels of the clitoris can be retracted easily.

In the diagram I exhibit I have endeavored to indicate the mechanism of the procedure, the necessity of the division of the infrapubic ligament, and the position and importance of the suprapubic triangular space. It will be observed that the separation of the symphysis, to the extent of two inches, increases the antero-posterior diameter least and only to the extent of about half an inch. while the transverse and oblique diameters are increased to about one inch. When the extreme divarication of four inches is obtained at the pubes, the antero-posterior diameter gains only one-sixteenth of an inch over that derived from the separation of two inches, while in the other diameters much additional space is obtained. The diagram also shows the slight separation of the anterior margins of the sacro-iliac synchondroses, especially when they yield simultaneously, when the divarication at the symphysis reaches three inches. The anterior separation at the synchondrosis is associated with the

approximation of the extra-pelvic portions of the ossa innominata to the sides of the sacrum. The chief mechanical advantage of the operation is due to the resulting mobility of the pelvic girdle, which is most utilized by the efforts of extraction being made alternately in different directions.

In order to perform the operation of symphysiotomy in an anatomical manner it is necessary to expose the entire surface of the pubes. I am aware that many gynæcologists advocate a very limited incision, but it is my impression that for the subsequent treatment of the wound, as well as for the safety of the operation, the longer incision is preferable. The incision through the soft parts, as in all operations in this region, had better be made from below upward, cutting immediately to the cartilage and abdominal aponeurosis and subsequently separating the muscles. In consequence of the possibility of an irregularity at the symphysis being present, it is expedient to make the incision through the cartilage, from above downward and from before backward, in the direction of the normal wedge-shaped space occupied by the cartilage. I exhibit a knife which can be used advantageously for the division of the cartilage. It was devised by the late Professor Syme, of Edinburgh, for his well-known amputation at the ankle joint. It is short, blunt at the point, and strong. In addition to the impossibility of using it in abnormal cases, Galbiati's sickle-shaped knife, which cuts from the posterior surface of the symphysis, is mechanically defective, as before it can be introduced between the bones it must assume an antero-posterior position. Owing to its great curve, the highest point of which is situated more than an inch from its chord, the bladder and associated delicate structures are unnecessarily displaced from the back of the pubes to a corresponding extent. After the interarticular fibro-cartilage is divided and the bones separated, the subpubic ligament becomes visible in the angular or "V" shaped space between them, where it can be divided by successive touches of the knife. When it is completely divided the walls of the interval become parallel, and by traction on the thighs the bones can be separated with facility.

In the subsequent treatment of the wound, it would be well to attempt to obtain closer union of the bones than that obtained by the ordinary superficial suturing of the

soft parts. A secure as well as an easy method of fixation is by the oblique insertion of double needles or "staples," to which additional stability can be given by a ligature, preferably of wire, applied around the projecting extremities.

I think it is unnecessary to dwell upon the limitations of symphysiotomy, beyond indicating that in an extremely contracted pelvis it is not likely to prove successful. In some instance, as in ankylosis of the synchondrosis, it may be necessary to substitute Pinard's operation of pubio-ischiotomy, in which the line of incision resembles that for obturator hernia. In ankylosis of the symphysis, pubio-ischiotomy might be performed on both sides. I shall not dwell upon the operation of synchondrotomy, as it entails unjustifiable mutilation, and I refer to it only as a procedure to be avoided. I shall conclude by alluding to one other point, the possibility of utilizing symphysiotomy in some cases in which Cesarean section might be performed successfully were it not that the head of the child had become impacted. In such critical instances symphysiotomy should afford a rapid solution of a serious problem.

Dr. E. H. Grandin.—I wish to present here to-night two patients upon whom I have performed the operation of symphysiotomy, in order to show that the operation causes no ultimate disability. The histories of the cases are as follows:

Case I.—M., age nineteen years, nullipara; at term. When I was called to this patient the membranes had been ruptured for four hours; the first stage was completed; the vertex presented well flexed in the left anterior position. Notwithstanding strong uterine contractions, the fœtal head remained wedged at the brim. The fœtal heart was 160 and irregular. The mother's pulse varied from 120 to 128. The pelvic measurements were: spines, eight and three-quarter inches; crests, nine and three-quarter inches; antero-posterior, about seven inches; conjugata vera (under chloroform), three and one-quarter inches. The pubes were deep, and the sacral promontory projected in a sharp spine.

Version being contraindicated, I applied the forceps, but was unable by judicious traction to bring down the head. I at once elected symphysiotomy, and made a primary incision extending upward three inches from the

pubic eminence. I separated the recti with my finger and inserted this under the symphysis. With an ordinary bistoury I made a section of the symphysis from above downward and from without inward to the subpubic ligament. The fœtal head being wedged at the brim interfered greatly with the severing of the inferior ligament of the pubes by the Galbiati knife. On completion of the operation the pubes separated to the extent of one and one-half inches, this space being increased to the extent of one inch during difficult forceps extraction. Deep silkworm-gut sutures and superficial catgut sutures were used to bring the tissues together. During the pubic section and in bringing the bones together the urethra was depressed carefully. The operation was performed February 11th, 1893. The convalescence was protracted by a mammary abscess, and by a mural abscess breaking through the line of incision.

On the tenth day a vesico-abdominal fistula appeared, which closed spontaneously. The patient was first out of bed on the fifteenth day, and walked for the first time on the thirty-fifth day. At present there exists close union at the symphysis and no mobility. As you see, the patient is able to walk and run and jump perfectly well.

This woman could only have been delivered otherwise by Cæsarean section, and symphysiotomy was elected in order to avoid still-birth and embryotomy of the living child. The child might have been dragged through by forceps, but dead, and at the expense of the maternal soft parts. The abdominal-vesical fistula was due to one of two causes. The fœtal head being wedged at the brim, the neck of the bladder may have been injured during the insertion of that clumsy instrument, the Galbiati knife; or, notwithstanding the care with which the urethra was depressed, the bladder was nipped by the pubic bones when they were brought in apposition.

Case II.—M., age twenty-three years, nullipara; four feet six inches in height; infantile pelvis with the following measurements: spines, nine and one-half inches; crests, nine and three-quarter inches; antero-posterior, six and three-quarter inches; conjugata vera (under chloroform), three inches. The patient came under observation about seven weeks before term. After consultation with my colleague, Dr. R. A. Murray, three separate attempts were made to produce premature labor. On

two occasions glycerin was injected into the uterus, the patient being in the Sims position and the pelvis elevated. The first time the attempt was absolutely futile; the second injection was followed by strong uterine contractions and a profuse discharge of serum. I next endeavored to induce labor by packing the cervical canal and the upper vagina with gauze. This also failed in the desired result. On February 20th, 1893, spontaneous labor set in. After thorough examination under chloroform the decision was reached that symphysiotomy best assured a living child. This was accordingly done and the child delivered by podalic version. The operation was similar to that in the first case, with the exception that the more slender Galbiati-Harris knife was used to cut the subpubic ligament. The fœtal head being movable above the brim, this very essential part of the operation was not difficult. The pubic bones separated during the extraction to the extent of two inches. The convalescence of the patient was uninterruptedly normal. She was out of bed on the twenty-first day, and walked for the first time on the twenty-fourth day. On March 23d, when examined in the erect position, there existed slight mobility at the symphysis, although there was close union. At present the patient walks well; there is slight motion at the symphysis. The child is alive.

This patient might have been delivered by forceps through protracted moulding of the fœtal head; very likely, however, with resulting still-birth, or, at any rate, with grave lesions due to pressure upon the fœtal brain. Here lies one of the great results to be predicted from symphysiotomy after it shall have been proven perfect in its ultimate results from the maternal standpoint. The wider my experience the more the truth is forced upon me that many a deficient intellect might be traced to protracted labor and the means resorted to for delivery—such as prolonged forceps traction through the pelvic brim or difficult extraction of the head after version. These cases are respectively the fourth and fifth in New York City, and are the first ever shown in public in the United States. Of the twelve operations in the United States to date, six have been performed by Fellows of this Academy.

H. J. Garrigues, M.D., followed with a paper on

THE RESTORATION OF SYMPHYSIOTOMY.

"*Una Fides, Altare Commune*" is the noble legend of this body. Medical science has in the course of time become so extensive that it has been found necessary for those who strive after the highest perfection of execution in one part of its domain, or who contribute most to the elaboration of a certain branch of it, to limit their original researches and their practice more or less exclusively to one group of organs or one kind of diseases. It is due to this wise division into specialties that medical science has taken such enormous strides forward in the course of half a century. But if it is impossible for a man to do original work in all directions, and if it is even hardly feasible to be sufficiently familiar with all medical science to practise it in such a way as to gain credit for himself and do justice to his patients, every physician who is imbued with that spirit which distinguishes the man of science from the mechanic and the money-maker wishes at least to know the most important discoveries, inventions, and events that are made and occur in any part of the vast field of medical science, in the widest sense of the word. As a help in the pursuit of this design, this Academy from time to time provides general meetings under the auspices of one of its sections; and thus it has come to pass that the President, having been made acquainted with the occurrence of an event of great importance and far-reaching consequences in the practice of the obstetrical art, has invited the Academy at large to listen to a report from the Section on Obstetrics and Gynæcology, and I have the honor to open a discussion by some brief remarks on the restoration of symphysiotomy.

History and Statistics.—The history of this operation is unique. It was proposed in 1768 by Jean René Sigault, a French student, and rejected by the Academy of Paris, but performed by him in 1777 as soon as he had graduated. It was hailed with enthusiasm, but soon fell into discredit and only survived in Italy, and was, per-

haps, dying out even there, when in 1866 Prof. Morisani took it up and became an ardent champion for it, but without finding any response outside of Italy until, in 1891, a pupil of his, Spinelli, demonstrated it to Pinard in Paris, who became much interested in it and performed the first operation outside of Italy, in 1892. Since then most other countries have followed in rapid succession.

The first symphysiotomy in this country was performed by Dr. Jewett in Brooklyn, and the first in this city by myself on December 30th, 1892. Dr. Robert P. Harris, of Philadelphia, has kindly furnished me the following statistics: From 1777 to 1860 (which may be called the old period) there were 100 operations, in which 31 women and 65 children were lost. Since 1866 till the end of 1892 there have been 173 operations. In 1892 there were, as far as ascertained, 66 operations in 9 countries, with 5 deaths. Since January 1st, 1886, which in some respects forms a turning point in the history of the operation, there have been in all 122 cases with 12 deaths, *i.e.*, nearly 10 per cent, but most of these were due to prior conditions. The rate in Italy has been 2 women lost out of the last 44 reported operations. Pinard has had 13 operations in as many months, without maternal loss. Twenty women operated on by him and his associates all recovered, and 18 children lived. In this country there have been 7 cases with 3 deaths.

Space Gained.—If the symphysis pubis is cut in a woman lying on her back with outstretched legs, the ends of the bones separate very little, only about half an inch; but if the joints of the hips and the knees are bent the distance is one and one-quarter to one and a half inches, and by pulling on the iliac bones this is easily increased to two and three-quarter inches without injury to the sacro-iliac articulations. But if the separation is carried as far as three and a half to four inches, one or both of these joints are torn open.

In consequence of the separation of the pubic bones,

a considerable change takes place in all the diameters of the pelvis, whereby it is rendered much more spacious in all directions, on planes supposed to be laid at right angles through the axis.

The antero-posterior diameters cease to exist, and instead a gap is formed in front which allows the presenting part to enter between the ends of the bones. If the vertex presents, the anterior parietal eminence enters in this way between the bones to such an extent that it has the same effect on the mechanical relations between head and pelvis as if the antero-posterior diameter had become one-quarter to three-eighths of an inch longer. In consequence of the yielding of the sacro-iliac articulations, which during pregnancy become more mobile, the distance from the centre of the promontory to the ends of the pubic bones increases the more these bones are separated from each other—an increase which, at the maximum safe distance of two and three-quarter inches between the ends of the bones, measures over half an inch. The transverse and the oblique diameter, and every line drawn from the middle of the promontory to a point on the anterior half of the ilio-pectineal line, increases from one-quarter to one-half of the distance between the ends of the pubic bones. At the safe maximum distance of two and three-quarter inches these lines gain, therefore, from about three-quarters of an inch to an inch and a half in length.

Indications.—Supposing we have to deal with a head of normal size, we can easily calculate the lower limit within which symphysiotomy is available. The biparietal diameter of the fœtal head at term is three and three-quarter inches. The distance from the middle of the promontory to the end of the pubic bone is elongated half an inch. The head enters to a depth of one-quarter of an inch or more, and consequently three-quarters to seven-eighths of an inch is gained. Taking into consideration the compressibility of the head to the amount of one-quarter of an inch, we may say that the passage will be difficult with a

conjugate of two and three-quarter inches, and easy above three inches.

As to the upper limit, it ought, in a flat pelvis, to be placed at three and a half inches, where the proper domain of forceps and version begins.

In a generally contracted pelvis, which practically offers the same resistance as a flat pelvis with a conjugate one-half inch shorter, I think it would even be proper to extend the upper limit to a conjugate of four inches.

Besides the narrowness of the pelvis, other conditions may become an indication for symphysiotomy. Thus it has already been used in the case of a tumor developed in the pelvic cavity, and it has been proposed for mento-posterior face presentations in which the chin cannot be rotated forward, and in cases of absolute narrowness of the pelvis if the child is dead, in order to facilitate craniotomy and embryotomy.

Besides measuring the pelvis the accoucheur must examine the mobility of the sacro-iliac joints by bending the hip and knee joints and turning the knees outward. An ankylosis of one of these joints is a contraindication to the operation.

Modus Operandi.—Two methods are used at the present time, the subcutaneous and the open. Morisani, of Naples, to whom we practically owe the survival of the operation, makes a longitudinal incision, one and a quarter inches long, in the median line, ending a half to three-quarters of an inch above the symphysis. Next he cuts sideways into the recti muscles, deep enough to make room for the index finger, which he introduces down to the lower end of the symphysis, and along which he slides a strong sickle-shaped knife, Galbiati's falcetta, and cuts from behind and below forward and upward. The urethra is held over to the right side by means of a rectal catheter. After delivery the wound is closed with sutures and dressed antiseptically, and the pelvis is surrounded by a roller bandage, which is rendered immobile by painting it with waterglass.

The open method has been introduced by Pinard, of Paris. It consists in making a longitudinal incision in front of the symphysis, extending it sufficiently above the symphysis to have easy access to the latter, and below to the root of the clitoris, or, deviating to the left of that organ, into the vulva between the labium majus and minus.

The subcutaneous method has the advantage of great simplicity of execution, and of having a small wound entirely removed from the lochial discharge, and it gives rise to less hæmorrhage. On the other hand, it has the grave drawback that if hæmorrhage occurs it cannot be properly attended to. As a matter of fact, most of the new operations have been performed according to the open method, with small variations upon which we cannot enter here.

It happens occasionally that the symphysis is ossified or has so irregular a shape that no knife can be drawn through it. In such cases it must be divided with a saw, preferably a chain saw.

The bladder may be so compressed between the head and the symphysis that it can hardly be distinguished, and may, therefore, be wounded; and both bladder and vagina may be caught between the ends of the pubic bones, if care is not taken to hold them back in closing the pelvis.

After having cut the symphysis Morisani leaves the expulsion of the child to Nature, or, if necessary, uses the forceps. The new operators prefer to deliver at once, and I think the latter is preferable and should be done, according to the rule that if the head engages forceps should be used, if it is movable above the brim version should be performed.

In a large number of operations the child was born more or less asphyxiated. Everything needed for its revival should, therefore, be kept in readiness. If the placenta does not follow the delivery of the child it should be removed before closing the wound.

In closing the wound some have brought the ends of the bones together separately, and even gone so far as to bore holes and unite them with three silver sutures. From my own case and many histories I have read, I conclude that all that is really necessary is to put the sutures in so deeply as to include half an inch of the sinewy tissue on both sides in front of the symphysis and the pubic bones. In closing the sutures the bones are brought together by pressure on the trochanters, and the legs should be stretched out, or even hang down on each side of the operator, which approximates the ends.

In order to keep the ends together I do not know of anything more convenient, and in every respect satisfactory, than rubber adhesive plaster, three broad straps of which are put around the trochanters and crossed on the abdomen above the wound. The wound should be dusted with iodoform and covered with iodoform gauze and gutta-percha tissue, and the dressing renewed every day, as it becomes soiled by the lochial discharge.

The patient is lifted on the bedpan, when needed, by holding her behind the trochanters. She should lie with outstretched legs, and the knees should be tied together.

She should be kept in bed for three weeks.

Prognosis.—The prognosis of the operation for the mother's life and the unimpaired natural gait may be said to be absolutely good if the above indicated limits are respected and strict antisepsis is used in the performance. It is true, even in the modern operations nearly ten per cent of women have died, but their death had no connection with the operation. In a few cases a vesico-vaginal fistula will form, but it will heal by itself or can later be closed by operation. For the child the prognosis is not quite so good as for the mother, several having died from fracture of the skull and many being born more or less deeply asphyxiated ; but the infantile mortality of ten per cent is much less than in difficult forceps or version operations, not to speak of operations by which the child is purposely mutilated and killed.

Relation to other Obstetric Operations.—By the revival of symphysiotomy the indications for nearly all the measures to be taken in order to overcome an obstruction preventing the birth of the child have been changed—a chief reason why every practitioner engaged in midwifery should acquaint himself with the scope of the new operation, even if he does not wish, or is not prepared, to perform it himself.

First of all, the revolting direct killing of the child in the mother's womb, or, what amounts to the same, the abstention from interference which awaits the natural death of the child and then mutilates its body, must be banished from the list of obstetric resources in places where it is possible to have symphysiotomy performed.

Induction of premature labor with a maternal mortality of over five per cent and an infantile mortality of over forty-three per cent[1] must likewise be abolished.

The improved Cæsarean section, which even in the hands of a Leopold shows a maternal mortality of eight per cent,[2] and which in the hands of miscellaneous operators has been attended by a loss of nearly twenty-six per cent of the mothers and over eight per cent of the children,[3] must be confined to cases in which the true conjugate measures less than two and three-quarter inches.

Symphysiotomy seems even called to supplant Porro's operation to a great extent, since experience shows that it can be performed successfully when the woman has been in labor for many days, whereas the maternal mortality in Porro's operation reaches the fearful height of fifty-seven per cent.

Even many forceps and version operations ought to yield to symphysiotomy, since these operations, performed with a true conjugate of less than three and a quarter inches, are accompanied by a great mortality both for mother and child, or lead to idiocy in the latter.

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[1] Wyder, Archiv für Gynäkologie, 1888, vol. xxxii., p. 60.

[2] Leopold, ibid., 1889, vol. xxxiv., p. 313.

[3] Caruso, ibid., 1888, vol. xxxiii., p. 255.

The operation has in several cases been performed twice on the same woman, the second operation proving no more difficult and just as successful as the first.

In the above we have supposed the child to be normal and the pelvis defective. It is evident that a normal pelvis with an unusually large or deformed child offers the same mechanical disproportion and calls for similar interference.

Little over a year has elapsed since Pinard performed his first operation, which was soon followed by others in nearly all civilized countries. The facts in regard to all details are fast accumulating. Soon we shall know more definitely when and how to operate, but the experience gained suffices already to warrant the declaration that the generalization of symphysiotomy is an event of the greatest importance and far-reaching consequences in the history of practical midwifery.

Dr. J. C. Edgar, in continuing the discussion, said: This subject has been so well covered by the previous speakers that I will only say a few words about the operation. It is apparent to every one that if this operation takes the place that has been claimed for it, particularly within the past six months, obstetrics will have taken a long stride forward. One question that has been asked me repeatedly regarding the operation is, Do we actually gain in the diameters of the pelvis by cutting through the symphysis? If any one is in doubt about this, let him read the report of the investigations made by Wehle at the Dresden Maternity, and he will be satisfied that there is an actual increase in the transverse, oblique, and antero-posterior diameters. Another question repeatedly asked is, Are there no ill after-effects to the operation? We are told that in pre-antiseptic days, and before the technique of the operation was perfected, there were a number of serious complications, such as fistulæ, accidents in the way of sepsis, etc. As regards actual union of the joints after the operation, I think it is impossible to give a positive prognosis that we will always, or even in the majority of cases, get firm fixation. My own experience with the operation of symphysiotomy is limited to a single case, and in this the

result has been rather a fortunate one: forty-one days have elapsed since the operation was performed, the woman is up and about, she is able to go up and down stairs and perform her regular duties. When I was called to see the woman she had been in labor for forty-eight hours or over without relief. The cervix was two-thirds dilated. The vertex presented. The patient was placed under ether, and repeated examinations revealed the fact that the conjugate diameter was not over three and a quarter inches. The case was decided to be a proper one for symphysiotomy. The child's head was movable above the brim, and, after estimating its size, I first decided to give the axis-traction forceps a trial, and used them long enough to find that the head would not engage. The question of version also came up. The membranes had ruptured five hours before, and from the estimated size of the fœtal head I immediately rejected the idea of version as not likely to secure a living child. The woman's family were opposed to any cutting operation, but finally, after some discussion, they consented to have symphysiotomy performed. The operation was extremely simple. The incision was made directly over the upper portion of the symphysis. There was little or no hæmorrhage, the recti muscles were found and separated, and the child's head was pushed up from the pelvic inlet. A curved bistoury was passed in under the guidance of the left index finger, and the joint cut from above downward and from behind forward, the point of the knife always being covered by the index finger. A male catheter had first been passed into the urethra and the latter depressed. Immediately upon cutting through the symphysis the bones separated one and three-quarter inches; this separation was probably increased afterward during the extraction of the child. Extraction was performed by podalic version, the anterior foot being first grasped and brought down. The placenta was immediately expressed. The urethra being still depressed, the bones were brought together and kept so by heavy silk sutures, two of them being introduced through the fibrous tissue of the joint. The woman made an uninterrupted recovery. Her highest temperature was 100.8°, pulse 100. The sutures were taken out on the eighth day. A rubber-plaster bandage was kept on for four weeks. Upon examination at that time some motion of the joint was disclosed, the two bones moving one on the other.

As regards the use of the silver-wire suture, I would rather hesitate to pass it through the bones, for fear of setting up necrosis. Experience thus far seems to show that we can get fair union of the joint without the use of silver wire or silkworm gut.

Dr. R. A. Murray.—As this subject has been discussed so fully, I will only make a few remarks. I have been an adherent to the operation of Cæsarean section for the relative indication, which holds a good place, and which I do not believe can be entirely supplanted by symphysiotomy. There are two or three points in connection with symphysiotomy which I wish to discuss as regards the use of the operation, not by the specialist, but by the general practitioner. I do not think that symphysiotomy will take the place of good forceps operations or good version operations where they are indicated. I do not think that because we have at our command an operation by which we can enlarge the pelvis we are justified ever in neglecting a careful examination of the pelvis and the child's head. If we are going to have good results from symphysiotomy we must have good surgeons to do the cutting and good obstetricians to determine when the operation is indicated. I think that one of the most important points in connection with the operation is that it not only enlarges the brim of the pelvis, but also the outlet. I think that in the future, where we have a bad position of the head and where the outlet is the obstruction, the operation will be done. When the child's head is so far advanced in the pelvis that we cannot do a version nor push it back, there is nothing left excepting a craniotomy, unless we can enlarge the pelvic outlet. To attempt to extract the child's head with forceps would be to kill the child. In such a case symphysiotomy is a very great aid; in fact, it is the only aid we have to save the child's life. This is a point that I have not yet seen brought out in any discussion or writings on the subject. In occipito-posterior positions where the labor has been neglected we may do symphysiotomy with advantage. In cases where the operation is necessary it should, if possible, be determined beforehand; this is possible with our private patients and in the hospitals; proper measurements should be made and turning resorted to.

I question whether we get absolute union of the joint after the operation. It is far better, however, that such firm union be not obtained, because if the patient again

becomes pregnant and the operation is repeated the chain saw would be required. Therefore, if we have a joint that is serviceable, and if the sacro-iliac synchondroses are fixed—which is really the important point—we would not quarrel with the operation.

Another point is this: The operation, to be successful, must be done antiseptically. The peritoneal cavity should not be opened, neither should the space behind the symphysis be opened, as it is apt to result in phlegmonous trouble.

Dr. H. J. Boldt.—I have only a word to say regarding this subject, and that is to sound a note of warning to the general practitioner. Cæsarean section has been performed in many instances when there was no necessity for it. Now we have our new operation, symphysiotomy, and unless the cases are most carefully selected it will often be performed when the necessities of the case do not require it. It is true that the mortality following the performance of symphysiotomy, in the hands of good men, is low, but, as Dr. Murray has said, it takes a good obstetrician and a good surgeon. As regards union of the bones after the operation, we do not ever get bony union; it is ligamentous.

Stated Meeting, May 18th, 1893.

THE ORGANIZATION, EQUIPMENT, AND METHODS OF WORK OF THE DIVISION OF PATHOLOGY, BACTERIOLOGY, AND DISINFECTION OF THE NEW YORK CITY HEALTH DEPARTMENT.

By HERMANN M. BIGGS, M.D.

My object in presenting a paper to the Academy of Medicine regarding some aspects of the management of contagious diseases, and the organization, equipment, and methods of work of the Division of Pathology, Bacteriology, and Disinfection of the New York City Health Department, is to furnish the medical profession of this city with more detailed information than they have, or could otherwise easily obtain, as to the plans, methods, and work of the department in these respects. The department desires that the profession shall become more familiar with its purposes and work, as for their proper performance the support, co-operation, and assistance of the medical profession are essential. Furthermore, the department is so earnestly endeavoring in every possible way to limit the extension of the contagious diseases, and to improve and increase the facilities for their management and care, that it appeals with confidence to the profession for their aid.

Until September, 1892, the work of disinfection was in charge of the Division of Contagious Diseases. To extend the work and to improve the methods of dealing with contagious diseases, the Board decided on September 9th, 1892, to create a new division, to be called the Division

of Pathology, Bacteriology, and Disinfection, and asked the writer to accept the position of chief inspector.

The complete organization of the division was not effected, however, until January 1st, 1893, when it assumed charge of all the work included under the heads bacteriology, pathology, and disinfection.

The force of the department at this time (January 1st) was increased by the addition of twelve medical inspectors and a number of lay disinfectors.

The oversight of contagious diseases treated outside of the hospitals falls to the two divisions—first, Contagious Diseases, and second, Pathology, Bacteriology, and Disinfection.

The Division of Contagious Diseases is in charge of Dr. A. H. Doty, chief inspector. The medical force consists of the vaccinating corps, the special medical inspectors on lodging houses, sixteen district medical inspectors, and four diagnosticians. The city is divided into sixteen districts, and to each district is assigned a medical sanitary inspector. Every case of contagious disease reported to the Health Department by the physician in attendance, in compliance with the provisions of the sanitary code, is at once referred to the district inspector of the district in which it occurs, and is immediately visited by him. It is chiefly the work of these district sanitary inspectors that concerns our present purpose, so far as the Division of Contagious Diseases is concerned.

The directions for guidance of sanitary inspectors assigned to districts are as follows:

Every case of contagious disease must be visited on the day the report of the case is received by the department.

School cards are to be at once sent to the principals of schools, directing them to exclude from school children residing on the same floor, when the case is in a tenement house; and to exclude all children residing in the house when the case is in a private dwelling or boarding house. (Scholars thus excluded from school are not again received

until notification has been sent of the termination of the disease and the disinfection of the premises by the department.)

If a case of scarlet fever, diphtheria, or measles occurs in a family living in a boarding house, or in an apartment of a tenement house having not more than two rooms, and in which family there are one or more children unaffected by the disease, the patient is to be examined, and, if the diagnosis is confirmed, the case is to be reported to the central office for removal to the hospital, unless it is deemed possible or advisable to attempt to isolate the patient in the apartment.

Where cases of these diseases occur in families occupying apartments of more than two rooms, and where there is a reasonable possibility of efficient isolation, providing the directions of the Health Department are carried into effect, then, if the family desire to have the patient remain in the house, the conditions are to be clearly stated under which the department will permit this to be done. (See general circular on contagious diseases.) A copy of the circular on contagious diseases, in the language which they can best understand, is to be delivered to the family in which sickness has occurred, and to each family in the house. The latter are to be notified of the existence of the disease in the house.

In case it is deemed wise to attempt to isolate the patient, a room is to be selected for the patient which shall be as much separated as possible from the rest of the apartment, and, if possible, shall have windows opening outside, not on an air-shaft.

The carpet is to be taken up and all unnecessary furniture removed from the room, and the sick person transferred to it with all furniture and other articles from the room already occupied which have been exposed to infection. Doors, transoms, or other openings, if any, which open on to the public hall, are to be sealed.

When a case is isolated the inspector must revisit the same at least once in four days, and make daily reports

to the chief inspector of all such cases isolated and his visits to the same.

If in subsequent visits to cases that have been isolated, in tenements and apartment houses, it is found that the directions of the department as to isolation and disinfection are not being carried out, then the chief inspector is to be at once notified of this fact. The inspector must see that proper disinfectants are provided and used by the family, excepting in the final disinfection of rooms and contents by the department disinfectors. If for any reason it should be necessary to have disinfectants furnished by the department during the course of the disease, special notice must be sent to the central office.

Whenever cases are found in public buildings, hotels, schools, apartments in rear of stores, or where manufacturing is being conducted, or when a case is to be removed, or when the body of a person who has died of a contagious disease is exposed and the rules of the Board of Health governing the same have not been complied with, or when a case of contagious disease is found which has not been reported by the attending physician, or when no physician is in attendance, the central office must be immediately notified by telephone.

Where cases of the above-mentioned diseases occur in *private* dwellings (not boarding houses) in which only one family lives, the inspector is not required to visit the case, unless requested to do so by the physician in attendance.

In cases of diphtheria the inspector is to call on the physician in attendance and request permission to make swab inoculations from the throat of the patient. The tubes thus inoculated are to be at once forwarded to the laboratory at 42 Bleecker street.

The following day a report of the result of the examination will be sent to the physician.

In all their relations with family and physician, inspectors should use exceeding care that nothing is done or said which can be considered discourteous or unprofessional.

Inspectors are directed not to examine patients unless requested to do so by the physician in attendance, or to interfere in any way with the conduct of cases, excepting as to isolation and disinfection, which must be always enforced excepting in private houses, and then the final disinfection is required.

On the termination of the illness in every case of contagious disease, the name and address of the patient are to be returned to the central office, and they are at once sent to the Division of Disinfection and the premises are disinfected.

The sanitary inspectors are required to subsequently verify the fact of disinfection and report the same to the chief inspector.

The above directions are issued to all district medical sanitary inspectors.

The organization of the Division of Pathology, Bacteriology, and Disinfection, with its force of employees and equipment, is essentially as follows:

The headquarters and laboratory are situated at No. 42 Bleecker street. The disinfecting station and stable are at foot of East 16th street, near the Willard Parker Hospital.

The force of employees, thirty in number, consists of the chief inspector, an inspector of bacteriology, an inspector and bacteriological diagnostician of diphtheria, an inspector in charge of the disinfecting station and the employees engaged there, four medical inspectors of disinfection, nine lay district disinfectors, five lay disinfectors for special work and ambulance service, three drivers, two disinfectors at disinfecting station, one engineer, one messenger, and one clerk, four wagons and horses.

At No. 42 Bleecker street a laboratory has been fitted up for bacteriological work the equipment of which is fairly complete and sufficient for carrying on any ordinary bacteriological examinations.

Inspector in Bacteriology A. L. Beebe, under the chief inspector, has charge of the laboratory, and Dr. F. W.

Lester has charge of the office situated there. The general direction of the work of the division is centred in this office.

The disinfecting station in East 16th street is a two-story building, twenty-eight feet wide by sixty-five feet deep, the ground floor of which is used for the purpose of disinfection, and the second floor for storage of disinfectants and for disinfected goods after disinfection, where for any reason they cannot be immediately returned. The designs for the station, in essentially the form in which it now stands, were furnished for the department by Dr. Prudden and myself some four years ago.

The first floor is divided by a corrugated-iron partition, sealed above and below into two parts, which have no communication with each other excepting through the disinfecting chamber. In passing from one side of the station to the other, one must go outside around the building. The purpose of this partition is to keep absolutely separate the infected side and the clean side of the station and their contents.

The floor on both sides of the station is of asphalt, and the rooms are arranged so that the walls and floors can be washed at frequent intervals with a disinfecting solution.

On the infected side of the station is a large cremation furnace for the destruction of all infected articles which from their nature cannot be disinfected, or which have become useless. There are also situated here tubs for holding disinfecting solutions for the disinfection of such articles as may occasionally be carried to the station, and which can be soaked in such solutions. (Articles which can be soaked in disinfecting solutions are not ordinarily taken to the station for disinfection, but are treated where found.) There is a sink furnished with three faucets for supplying, respectively, hot and cold water and carbolic acid solution. There are also hose connections from a tank (later described) for supplying a corrosive sublimate solution. Near the partition which separates the two

sides is situated a large iron wash boiler, six feet in diameter by three feet in depth, with proper supply and discharge pipes and steam connections for heating the contents. A hood from the partition separating the two sides of the plant is extended over to the centre of this boiler, and the cover of the boiler opens on one side into the infected room, and on the other into the clean room of the station, so that articles which are to be disinfected by boiling can be introduced on the infected side and withdrawn on the clean side. The disinfecting chamber is situated near the centre of the building, and arranged so that the doors at one end of it open into the infected room, and the doors at the other end open into the clean room. The partition separating the two rooms is flush with the outer end of the chamber. The floor of the chamber is about two feet above the floor of the station, and a large platform extends out from the doors of the chamber at either end. This supports the tracks which extend through the chamber and upon which the car (later described, for carrying articles) runs.

On the clean side of the plant there is:

1. A thirty-horse-power boiler for supplying the steam used for all purposes in the station.

2. A Sturtevant exhaust fan for rapidly circulating the steam and hot air in the disinfecting chamber while disinfection is in process.

3. A small engine for supplying power to run the exhaust fan.

4. A large boiler for supplying hot water to all parts of the plant.

5. Two large elevated wooden tanks, each having a capacity of one thousand gallons; one of which contains a solution of bichloride of mercury, one part in one thousand, and the other a solution of carbolic acid, one part in forty. These tanks are supplied with proper connections for carrying disinfecting solutions to the sink and to all parts of the station.

6. The disinfecting chamber. A somewhat more de-

tailed description of this is necessary. It is a brick chamber, inside measurements of which are: length seven feet, height six feet, width six feet. It has an arched roof, and doors at either end. One pair of doors opens into the infected side of the station and one into the clean side. The chamber is lined above, below, and on the sides with coils of steam pipes. The coils below are separated from the interior by a perforated iron floor, those on the sides and top by a heavy iron wire netting. Under the perforated floor, in addition to the coils of steam pipes, there is a cross composed of one and a quarter inch pipe, containing a large number of perforations. This cross is connected with the steam boiler and serves to introduce live steam. In the top of the chamber are nine openings, each six inches in diameter. From these openings pipes covered with asbestos extend to the exhaust fan, and from the latter similar pipes extend under and open into the floor of the chamber; by means of the exhaust fan the contents of the chamber, whether steam, or dry hot air, are drawn out from above and introduced again below and are rapidly circulated. Forced currents are thus produced in the chamber. As the pipes are covered with asbestos, there is no considerable loss of heat in this procedure. An apparatus for heating air is also connected with the fan, and it is arranged so that the steam is replaced by hot air, when the process of disinfection is completed, to prevent wetting of the contents by the condensation of steam. There is also a pipe leading from the discharge of the exhaust fan to a ventilator, so that the steam withdrawn from the chamber is discharged outside of the building, while hot air is drawn through the heater into the chamber to replace it.

The doors of the chamber are double, fill completely each end, are six inches in thickness, made of wood, lined inside with tin, and the interior filled with sawdust, designed to be non-conductors of heat.

Upon the tracks which extend through the chamber is a car fitted with galvanized-iron racks and hooks. The

car with the racks fills the whole interior of the chamber. This is loaded on the infected side, is run into the chamber, the contents disinfected, and it is then withdrawn and unloaded on the clean side.

The department has just received from Germany a disinfecting apparatus, mounted on wheels, of the largest size and most approved modern design. This is for use especially in connection with any localized outbreak of contagious disease or in connection with temporary hospitals.

The stable, also situated at East 16th street, is for the accommodation of the horses used in transporting goods to and from the disinfecting station.

So much for the force and equipment of the division.

The work of the division, as the name indicates, may be brought under three general heads—first, pathological; second, bacteriological; third, disinfection.

The pathological work is comparatively limited in character, and is confined to the post-mortem examination, and, when necessary, subsequent microscopical examination, of cases of suspected contagious disease, where the diagnosis has not been satisfactorily established during life. It has generally been limited to suspicious cases of epidemic cholera and typhus fever.

The bacteriological work of the division is of three kinds. 1. That for establishing the diagnosis in doubtful forms of disease, as in diphtheria and cholera. 2. Investigations as to the germicidal value of various disinfectants and the efficiency of various processes of disinfection. 3. Bacteriological analyses of water, milk, etc.

1. The most important work under the first heading, the biological diagnosis of disease, thus far performed was that done during the outbreak of epidemic cholera in New York during September and the early part of October, 1892. It is not necessary to speak more in detail of this, as a report of the work done at that time has already been published. I desire, however, to direct special attention to a new departure that has been determined upon

by the Board, that of making regular and systematic bacteriological examinations for the diagnosis of diphtheria. So far as possible it is desired to make bacteriological examinations in all cases of diphtheria reported, but the wishes of the attending physicians will be consulted, unless the case is to removed. All cases to be removed to the hospital will be examined before removal. These investigations are certain to lead to an accumulation of valuable data regarding diphtheria and allied affections. They will also be of great service in conserving the resources of the department and in limiting its work to cases of true diphtheria. It has been thoroughly demonstrated that it is impossible to make an accurate differentiation between diphtheria and other pseudo-membranous affections of the throat from the clinical history and the anatomical appearances. Dr. W. H. Park, who has been appointed inspector, has had special training and experience in this work. The appointment, establishing, as it does, the policy of the Board regarding this disease, is a step in harmony with its previous action regarding the diagnosis of Asiatic cholera, and is abreast of the most modern investigations.

2. The experimental work in disinfection consists of investigations into the germicidal value of such disinfectants and processes of disinfection as may come up for consideration. These investigations have thus far been chiefly confined to, first, an elaborate series of experiments on the germicidal value of sulphurous acid gas, as derived from liquid sulphurous acid and from burning sulphur; second, to a series of experiments to determine the germicidal value of so-called "electrolyzed sea water"; and, third, a series to determine the efficiency of the routine method of disinfection employed at the disinfecting station in East 16th street.

3. Weekly bacteriological examinations are made of Croton water.

Practical Work in Disinfection.—This may be brought under two heads. 1. The work done on the premises

where the cases of contagious diseases have occurred. 2. The disinfection of articles removed to the disinfecting station for that purpose. For the disinfection of premises the city is divided into nine disinfection districts; to each of these districts a disinfector is assigned. Every morning a list is furnished him which includes all the cases of contagious diseases in his district which have been reported during the preceding twenty-four hours by the district inspector of contagious diseases, as terminated either by death or recovery; in other words, a list of all those premises in which the rooms occupied by cases of contagious diseases have been vacated and which are ready for disinfection. All such places are visited by district disinfectors, and are treated in accordance with a definite formula. In addition to these district disinfectors there are special disinfectors, whose duty it is to deal with the special cases requiring immediate or unusual treatment, as they come up each day. These special cases include all cases of typhus fever, cholera, small-pox, and special cases of scarlatina or diphtheria, which occur in hospitals, lodging houses, etc., and those cases occurring in private houses which for special reasons require immediate and careful or unusual treatment. These special disinfectors usually work under the direct supervision of medical inspectors of disinfection. There are also special disinfectors detailed with the ambulances for day and night service, so that the premises may be immediately disinfected when cases are removed to the hospitals.

There have been established in the city twenty supply depots for disinfectants. Disinfectants are delivered at these depots by the wagons, and the district disinfectors obtain their supplies of disinfectants from these depots. It would be otherwise impossible for them to carry what is required for their work. Each special and district disinfector has a kit for carrying sulphur, bichloride of mercury tablets, paper, paste-pot and brush for sealing doors and windows, a pair of large scissors, a tape measure,

alcohol for igniting the sulphur, an alcohol lamp and pan to hold water for boiling to moisten the air, a suit of overalls to be worn while disinfecting rooms, pans for holding the sulphur—in a word, all the articles which it is necessary for him to have in his work. The instructions to disinfectors are specific and include the following:

Bed linen, blankets, counterpanes, towels, napkins, personal clothing, which can be washed and which have been exposed to infection, are to be put in a bichloride solution, strength one part in one thousand. Mattresses, pillows, comfortables, etc., from infected beds, and all clothing, curtains, draperies, etc., which cannot be washed, are to be wrapped up in sheets, wrung out in the bichloride solution, and left for removal to the disinfecting station. Rugs, carpets, etc., should be folded up ready for removal. When carpets are still fastened to the floor they must be thoroughly sprinkled with a bichloride solution. The floors, baseboard, etc., are to be washed with a similar solution. While these directions are being carried out water should be kept boiling in the room to moisten the air. The cracks around the windows, doors, etc., and all openings, are then to be sealed up by pasting paper over them, and sulphur is ignited, the amount being equal to three pounds to the thousand cubic feet of space. The outside door is closed and sealed, and directions are left with the tenants that the room shall not be opened in at least eight hours. The following day the wagons call at the houses and remove to the disinfecting station such goods as have been prepared for removal. Upholstered furniture is not treated, excepting by fumigation, as the Health Board feel that they are not prepared to compel its destruction, and there is no method of disinfection, excepting that of fumigation, which can be employed. After fumigation the owners are advised to have it reupholstered.

To control the work of the district disinfectors and insure their compliance with directions, the medical inspectors of disinfection make inspections at irregular

intervals, as may be directed from headquarters, of the houses that have been subjected to treatment by the disinfectors, and the results of these inspections are reported daily to the office. Each disinfector is held personally responsible for the character of the work which he performs, and is conscious that some of the disinfections which he makes each day will be subsequently inspected by the medical inspectors.

2. *The Work at the Disinfecting Station.*—Goods are brought to the disinfecting station by the wagons for infected materials, and are treated in accordance with the directions of the medical inspector in charge. Goods which from their nature cannot be efficiently disinfected, or which it is not practicable to disinfect (as cotton comfortables), are ordinarily destroyed. Straw and husk mattresses, cotton pillows and mattresses, when received from lodging houses or tenement houses in a filthy condition, are cremated. Other goods are exposed in the disinfecting chamber to steam for one hour and to dry heat for one hour. They are then removed and returned to their owner by wagons used only for disinfected materials. The average temperature obtained in the chamber during the process of disinfection is 260° F.

To avoid any communication between the men employed on the two sides of the plant, those upon the infected side are furnished with brown overall suits, and those upon the clean side with white overall suits. The wagons transporting infected goods are painted yellow with black curtains, and those for disinfected goods are painted white and furnished with white curtains.

There were 16,385 pieces received at the disinfecting station for disinfection or destruction during the first quarter of this year. When it is noted that articles which can be soaked are disinfected on the premises, and that those which go to East 16th street are composed largely of mattresses, pillows, comfortables, carpets, heavy clothing, etc., this number, 16,385, will be seen to represent a large amount of work. The work is increasing.

Every ambulance, after having discharged its patient at the Willard Parker or Reception Hospital, reports at the disinfecting station for disinfection. The blankets are removed for disinfection, and clean blankets are supplied. The mattresses, which are covered with rubber, the floor, and the sides of the ambulance for about two feet up, are washed with a bichloride solution. The ambulance drivers are supplied with rubber coats, which they are required to wear while carrying patients, and these are also sponged in the same way with a bichloride solution after each trip.

One of the questions which have given me much concern has been the disposition of carpets from rooms occupied by cases of contagious disease. They are only second in importance to the bedding in the question of disinfection. The department cannot undertake to remove all carpets, as this would require a much larger force than is available. In many cases the owners will not take them up, and they cannot be compelled to do so after the cases have recovered. Further, they cannot be efficiently disinfected on the floor unless they are soaked with a bichloride solution, to which process the owners often object. We have found a general solution of the problem by requiring that in cases of contagious disease the carpets shall be taken up at once during the illness, rolled up, and left in the room. This diminishes the chances of dissemination of contagion by dust during the course of the disease, and the floors can be mopped. After the termination of the disease the room can be easily disinfected and the carpet removed with the bedding to the disinfecting station.

This completes the description of work of the Division of Disinfection. But do not let me be understood as saying that the process followed since January 1st is as I have described it; the work as described is the work of to-day and is the result of four months' experience. Important additions and changes have been made within two weeks, and more will undoubtedly follow.

The character of the work is improving week by week

as time and experience develop a clearer appreciation of its requirements.

The efficiency of the department's work will be improved if physicians will at once notify the chief inspector of contagious diseases, or of bacteriology, pathology, and disinfection, of any faults or omissions of employees coming to their notice.

Complaints are often made publicly by physicians regarding the work of inspectors and disinfectors, of errors in diagnosis, of incomplete isolation and disinfection. Regarding these let me suggest that if they are directed to the Board, and not to the public, they will receive careful consideration and will be of great service.

This description of the methods followed by the Health Department is not presented to you as an ideal system designed for your edification, but simply for your information as to what is now being done, with the hope that the work may be rendered more effective through suggestions and through the intelligent co-operation and assistance of the physicians of this city. It is within their power to render very material aid to the department in its endeavors to limit the extension of contagious diseases, and the department most earnestly desires this aid.

DISCUSSION.

Dr. E. G. Janeway.—I feared that I would not be able to attend the meeting this evening, and am not prepared to discuss at length the paper read by Dr. Biggs. I desire, however, to express my sense of appreciation of the wisdom evinced in the establishment of the Division of Disinfection, the workings of which were detailed in Dr. Biggs' paper. I see that Dr. Bryant, the late Health Commissioner, is to be present this evening, and he, better than any one else, can tell us if there be any occasion to defend the establishment of this bureau. The improvements in the methods of disinfection, and the thoroughness with which the work is carried out, will, I am sure, commend themselves to all. In New York City, as it becomes each year more dangerously overcrowded, there will be a constantly increasing necessity for the employment of

every possible means to prevent the spread of infectious diseases. Moreover, if it removes to quarantine persons who are suffering, or supposed to be suffering, with a contagious disease, it is bound to exercise every possible care in the diagnosis. This is done, in the case of diphtheria, by means of bacteriological and pathological investigations. I am sure that the members of the Academy will aid the Board of Health by every means in their power to carry on this work.

DR. A. JACOBI.—It was only at a late hour that I learned that I was to participate in this discussion, and I am hardly prepared to speak here to-night. Still, we are all familiar with some of the features of the subject, and I take pleasure in making a few remarks regarding it. A number of years ago, at the suggestion of Dr. Bryant, then Health Commissioner, this Academy appointed a committee to co-operate with the Board of Health. Dr. Bryant read a paper before you entitled "How Can the Profession Aid the Board of Health?" You all know the history of that committee and the conferences that have been held since by it. The Board of Health and the public are certainly indebted to the co-operation of the profession as represented by that committee. If the Health Department at that time was indebted to the profession, we are to-night indebted, and considerably so, to the representative of the Health Department for the very valuable information he has given us.

I am positive that nobody knows better than Dr. Biggs that, even with all this machinery, it is still impossible to do as much good as is intended. That is to say, we are still unable to fight contagious diseases as they ought to be fought. Dr. Bryant has himself said that as long as twenty-four families live over an area of twenty-five square feet it is impossible to isolate cases of contagious disease. If there is one case of scarlet fever or diphtheria in such a house there will certainly be more. You go into such a house and see a dozen children playing around the staircase, and you know that within a fortnight some of those children will be taken down with the disease. In a civilized community such a state of affairs should not exist. It should be impossible for a family to be content to live in one or two rooms. That was the complaint many years ago, that is the complaint to-day, and it will remain so. I repeat the request to you to-night which I have made before, and that is that those of you who have

pecuniary influence, who can command the wealth of a large number of people, should employ it toward the inauguration of some scheme by which it will be possible to temporarily remove poor children who are not sick from a house that has become infected. Otherwise it is impossible to avoid contagion. It takes a long time before the germ of diphtheria is removed from an infected house. If, with the same facility that a sick child is now removed from an infected house, the well children could be removed to a clean place to remain for a week or two, I am positive that a large number of contagious diseases would not occur and that many lives would be saved. Every year in this city we lose from two to three thousand children from scarlet fever and diphtheria. These are avoidable diseases, and we ought to devise ways by which they can be avoided. I believe that refuges for well children, such as I have referred to, could easily be established.

I wish to make another statement that I have long had in my mind. There is a class of houses in this city over which the Health Department has no command whatsoever. I refer to the first-class hotels. I know you will coincide with me that it is quite a common occurrence for diphtheria or scarlet fever to break out in a family occupying a suite of rooms in one of the large hotels. Only in a few such instances, when the family are strangers and the proprietor of the hotel does not particularly care for them, is such a patient removed. Otherwise the patient is allowed to remain. What becomes of that suite of rooms after the patient has recovered or died and the family have gone away, I do not know. It is very probable, however, and I have been in a position to prove it in some instances, that nothing is done to that suite of rooms. The floors are not washed, the carpets are not removed or steamed, the walls not disinfected, and the consequence is that another family moves in and you have the same thing over again. I have in the course of years seen diphtheria break out in the same suite of rooms year after year in expensive hotels, always in different families, perhaps strangers who came to the city for enjoyment and change of climate and so on. That is a point to which I desire to call the attention of the Health Department. I believe there are others here who can testify to what I have said. I know that in some hotels certain floors are infected while others are safe. I

have seen diphtheria year after year for three or four years in the same suite of rooms of a certain hotel.

There is another point to which I called the attention of Commissioner Bryant some time ago. We have in this city a class of tenement houses, under the name of flats, which contain three or four families on each floor from top to bottom. They are tenement houses under another name. In such a house, after the occurrence of a case of diphtheria or scarlet fever, the carpets are swept and then this dust and dirt is thrown out of doors. I believe these so-called flat-houses with Brussels carpets on the hall stairways, etc., need looking after.

The remarks I have made above are simply in the line of suggestions. It is the only way in which I can pay my respects and show my gratitude for the excellent paper we have listened to to-night.

DR. J. D. BRYANT.—I noticed, a day or two since, on my return from the country, that my name had been included among the list of those who were to discuss the paper read here this evening. It is like carrying coals to Newcastle for me to take part in discussing the subject which Dr. Biggs has brought before you, particularly in regard to its intricacies or the importance of it; in fact, in regard to anything relating to the minutiæ which he has depicted. The organization and equipment of this division of the Board of Health has been my dream for the past five years. In order to appreciate this matter thoroughly it is well to compare the opportunities of the Health Department of to-day with those in 1887. You will remember, I fancy, that in the fall of 1887 cholera appeared in the port of New York. While much was then said about the necessity for disinfection, yet the only disinfecting apparatus the department had at that time was a barrel and a small "smoke-house"-like structure; later a torpedo-shaped tank turned endwise, into which the material to be disinfected was thrown, was received. This, in addition to sulphur fumigations, were the only facilities at our command. At that time Mr. Hewitt, then mayor, promptly and gladly allowed the Health Department $3,600 with a view to the establishment of a proper disinfecting apparatus. The department then called upon Drs. Prudden and Biggs, and asked them to devise an apparatus which would be fully up to the highest scientific standard, as regards efficiency, for disinfecting purposes. The outcome of that request was the apparatus which

Dr. Biggs has described to you to-night, although it has been much improved since being under his charge.

So far as facilities for isolating and keeping under proper surveillance such persons as may have been exposed to contagious disease were concerned, the department was limited then to the Willard Parker Hospital, and to the small Reception Hospital which has since been reduced to ashes to make way for a much more pretentious and useful building. At North Brother Island the department had the pavilions, five in number, and the Small-pox Hospital. Since then three large pavilions have been erected on North Brother Island, in addition to a large number of tents, so that ample accommodations exist there for such infectious diseases as are of common occurrence within the city limits. More than that, in view of the fact that cholera is impending, all of the land at the foot of East 16th street on the north side has been secured, and rapid and satisfactory steps are being taken to put it into shape to accommodate numerous patients. I speak of these things in a reminiscent way, to show you the great difference between the facilities of the department in 1887 and 1893.

There is one thing, however, Mr. President, that this city needs above all things, and Dr. Jacobi will bear me out in the assertion. The doctor (Jacobi) referred to the presence of sick guests in hotels. He might just as properly have referred to the presence of a sick guest in your house or mine, or in any other than a tenement house. When a case of infectious disease happens in such a house, the patient must either remain there or be removed to North Brother Island and subjected to the same treatment as is given those who are sent there from the Sixth, Tenth, or Thirteenth Ward. That treatment is good; no one can gainsay that. Still, it is not first-class, yet the city contains no other accommodations for such patients. A building should be erected for the reception of patients suffering from contagious disease who can afford to pay for better and nearer accommodations than North Brother Island affords, and the person who succeeds in establishing such an institution will render the public, the profession, and humanity a great service. The establishment of such a hospital, placed in charge of the Health Department and controlled in its general management by that department, but nevertheless affording the physician an opportunity to treat his patient there, will be the next

great advancement in the treatment of contagious diseases in this city.

The organization of the division of the Health Department which has charge of the prevention of the spread and the eradication of contagious diseases, and the placing of it under the control of the gentleman who has just read the paper of the evening, I regard as one of the best and most fortunate steps which the department has yet taken. It requires, however, more than his personal efforts to make it a success. It requires the hearty and loyal co-operation of every member of the profession. Instead of making complaints one to another, as is often done by physicians, regarding what they denominate "the impertinence of the Board of Health," physicians should communicate directly and promptly with the Commissioners of Health when anything goes wrong. There are not a few physicians who regard the entrance of the health inspector into the sick-rooms of his patients as an impertinence. It would be an impertinence if it were done in the manner which these practitioners assume it is done. No sanitary inspector enters the sick-room for the purpose of making a diagnosis. He has nothing to do with the diagnosis, providing the patient be properly isolated. If that has not been done, then he has to do with the diagnosis, for the public welfare depends on it. The question of diagnosis, when the public welfare is not at stake, is one which concerns the patient and the attending physician only—not the physician of the Board of Health.

Fifty per cent of the patients who are sent to the Willard Parker Hospital and are supposed to be suffering with diphtheria have not that disease. The diagnosis in these cases is first made by the attending physician and then confirmed by the expert inspector from the Health Department. The patient then goes to the house physician, who approves the diagnosis. Now Dr. Park comes in with his bacteriological investigations, which show that from fifty to sixty per cent of the cases sent there are not suffering with diphtheria. I wish to bespeak for Dr. Park the hearty co-operation of the profession in his efforts to distinguish between diphtheritic and non-diphtheritic affections of the throat. In these efforts you may perhaps be occasionally annoyed by the inspectors—by the methods they employ or the manner of their address—but in that case all you have to do is to simply put yourselves in their places, as I have often told them

to put themselves in yours. Whenever it shall seem to you that they have overstepped the limits of medical propriety—which, I think, outwears every other form of propriety—then report the fact to the Commissioners or to the chief inspector. Report the matter as you believe it to have been, not as interested or prejudiced friends told you it was. I believe you will ever find that not only the Commissioners of the department, but likewise the chiefs of divisions, will extend to you every consideration that your profession exacts, so long as it does not conflict with their duties as sworn officers of the public health.

DR. J. W. BRANNAN.—There is little for me to add to this subject, which has been discussed so thoroughly by Drs. Biggs and Bryant from the standpoint of the Board of Health, and by Dr. Jacobi from the standpoint of the practitioner. In connection with the importance of a bacteriological examination in throat affections in children, I wish to relate the following experience. About three weeks ago one of my own children had a very suspicious-looking sore throat. The child had been subject to attacks of tonsillitis. It was not very ill and I treated it in the ordinary way, when suddenly, in the evening, the exudate took on a more serious character. Dr. Park, who lives near me, came over at my request with his tubes. From the appearance of the throat he thought it was not diphtheria; he said it looked very much like the fifty per cent Dr. Bryant has referred to. He took with him some of the exudation and the next morning informed me that the case was not one of diphtheria. I think one such personal experience as the above would convince any one of the wisdom of making a bacteriological examination in these cases.

It seems to me that separate hospitals should be erected for the reception of each variety of infectious disease. This has been spoken of in England. As it is, patients who are suffering with the milder forms of infectious fevers, upon removal to the hospital run the risk of being infected with a more serious disease. This is especially so with scarlet fever, in which the throat is rendered more susceptible to diphtheritic infection. I do not know how often this occurs, but I think that it is a possible occurrence. At North Brother Island I noticed that some of the patients suffered from a combination of infectious diseases, measles, scarlet fever, diphtheria, etc., and this may be due to the fact that all the patients are conveyed

in the same ambulance, in which some of the germs may still remain in spite of disinfection.

DR. T. M. PRUDDEN.—I do not know that I can add anything of value to this discussion, and will simply say a few words regarding some scientific aspects of it. As we look back over the careful and painstaking work that has been done in bacteriology during the last decade, we see that the main great practical outcome of it has been that we know now that a very large measure of prevention of disease, especially of contagious disease, is possible. It is possible in a measure which was hardly dreamed of ten years ago. Another thing which has come to us is the promise, or perhaps the hope, that the experiments which are being made will in the end lead us to some definite method of curing all these contagious diseases when once an individual is stricken. Unfortunately there are many doctors who do not do everything in their power to prevent the spread of infectious disease. That is largely what the Board of Health is doing, and it is a matter of satisfaction to the members of this Academy to see how closely the Board of Health of this city is in touch with the latest scientific investigations. I am rather disposed to think that the new department which the Board of Health has instituted for the purpose of diagnosing diphtheria marks an epoch in the practical application of science. It is not an indefinite promise, either. I do not think enough has been said about it. Last fall we saw what the Board of Health could do in the management of acute infectious disease in accordance with the new views in bacteriology and disinfection. I do not think we fully realize what a great achievement it was to almost immediately suppress a series of outbreaks of Asiatic cholera in New York.

DR. G. M. STERNBERG.—I had not expected to discuss this subject, but it is one of such importance and such interest that I cannot hesitate to say a few words. I am certainly very much pleased to learn how much has been done and how complete an organization you have in New York. I had no idea that so thorough an organization had been effected. Unfortunately, in the efforts of the Health Department, they are obliged to combat ignorance on the part of the public, and even on the part of medical attendants when they are irregular, as a certain number of them are. I shall look forward with much interest to the result of this work, and I trust that at the

end of a year somebody connected with the department will give us a tabulated statement of the results achieved as compared with previous results.

With reference to disinfection in cases of diphtheria, I must say that I have not very much confidence in sulphur dioxide fumigations. The germs that may be embedded in true diphtheritic membrane retain their vitality for a long time. The cholera spirillum it is not so difficult to destroy. When a bit of diphtheritic membrane happens to get into the carpets, it is extremely doubtful whether its vitality can be destroyed by sulphur dioxide fumigations.

One point with reference to the conveyance of infection. We are well aware of, and try to guard against, the infection that is carried by persons who enter the sick-room —nurses, doctors, etc.—but we do not know how often infection is carried by domestic animals. Dogs and cats should be excluded from the sick-room. Another possible means of conveyance is by flies. These are beyond our reach, but I am very much disposed to think that some of the very remarkable isolated cases of cholera may be due to the conveyance of the disease in that way. It has been shown that flies that have fed on the excreta of the disease may contain living cholera spirilla. Furthermore, the spirillum will cling to their feet and may thus be conveyed to meat, etc. Some cases in my own experience have led me to think very seriously that such a method of infection is by no means impossible. The first epidemic of cholera I ever encountered was out on the plains of Kansas in 1867 at a little frontier post. We had no railway communication with the outer world. It was an island in that prairie ocean. Cholera had appeared in New York the year before. It had gradually extended westward until it reached Jefferson Barracks, Missouri, from whence it was carried by a detachment of colored recruits. At our camp we were all in perfect health. Among the recruits there were a number of cases of choleraic diarrhœa, but the excreta were not allowed to contaminate our water supply. The first one who fell sick was a man in the butcher-shop, and after him another man in the same shop, which was situated up-stream from where the cases of choleraic diarrhœa had occurred. All the cases were attended in my post hospital, and the mortality was considerable. Neither the nurses nor I became infected with cholera. We took charge of the excreta, which were

subjected to such disinfection as we knew about at that time. In my own kitchen, however, my servant-girl, who did not go out at all, contracted cholera and died. She handled the meat that came into the house. I would like to ask Dr. Biggs whether there is any possibility of tracing out such a method of infection in any of the cases that occurred here last summer.

DR. B. ROBINSON.—I wish to relate a case that came under my observation a few weeks ago. I was called to see an English gentleman at one of the hotels in this city, and found him walking about the hotel. He complained of a sore throat, and on inspection I found that both tonsils were covered by a false membrane. I suspected diphtheria, and within an hour's time had Dr. Doty see the case, who confirmed the diagnosis. We had the choice of keeping him in a small room at the hotel with two trained nurses, or else sending him to the Willard Parker Hospital. There he was taken, and I believe he is there now. Before any bacteriological examination was possible he was removed to the hospital and placed in a room occupied by a child suffering with diphtheria.

DR. J. D. BRYANT.—Dr. Brannan spoke of mixed cases he saw at North Brother Island—patients with measles, scarlet fever, diphtheria, etc.—and stated that such mixed infection might be due to the fact that these patients were removed in the same ambulance. I think it proper to say that the mixed cases he saw were reported as such, and taken in charge of by the Health Department as such. Therefore the diseases were not contracted in the ambulance, the disinfection of which is believed to be perfect.

DR. A. JACOBI.—I think that by this discussion we have advanced very materially and we are now prepared to express some conclusions. We have learned of late that the Willard Parker Hospital is no longer sufficiently large to take care of all the cases that are sent there. The class of cases that are admitted there should not be grouped together promiscuously. They ought to be separated. Six months ago I said on this floor that we ought to have half a dozen Willard Parker Hospitals in this city. There should be one for cases of bacillus diphtheria and another for cases of streptococcus diphtheria. The latter disease is by no means such a trifling affair as has been stated here, for, as is well known, it frequently gives rise to the most intense sepsis. I think it will also

be found that the streptococcus diphtheria is a contagious disease; it will not do to trifle with it simply because the exudation does not contain the Löffler bacillus. I have seen many cases of it this past winter. Institutions are also required in this city, no matter who establishes them, for the reception of strangers who are taken with infectious diseases. It is not necessary that the Board of Health should establish them, but it is necessary that the Board of Health should have control of them. Those of you who control money, or who have friends who are millionaires, can easily get up a syndicate for the establishment of such a private hospital, just as private hospitals are established for other purposes. I believe this would prove a very good investment for moneyed men. I believe there are scores of families who would be glad to avail themselves of them.

Dr. D. B. St. John Roosa.—I desire to express, in the first place, the satisfaction of the Academy that the Board of Health has sent so distinguished a representative to present to us the methods they have adopted to preserve the health of the city and prevent epidemics. I will also take this opportunity to bring up a subject which, possibly, has not the importance of small-pox, or cholera, or diphtheria, but which does possess an importance which has been utterly overlooked—namely, contagious ophthalmia. When a man dies that is the end of him, but when a man becomes blind the city must take care of him for the rest of his life. There are no accommodations in this city worthy of the name for any one who is unfortunate enough to contract purulent ophthalmia. From motives of charity we have set aside one ward at the Manhattan Eye and Ear Hospital for the treatment of these patients, who are refused admittance into any other hospital in the city, for good reasons, I think. With us it was done as a matter of sentiment, and we are not, I fear, doing the greatest good to the greatest number by admitting them.

Dr. H. M. Biggs.—As regards the value of sulphurous acid as a disinfectant, we have not yet reached a satisfactory conclusion. We have arrived definitely at the result that sulphur dioxide (SO_2) is not efficient as a disinfectant without moisture, no matter how strong. With moisture it is fairly efficient. In all cases where sulphur is burned we use at least three pounds to the thousand cubic feet, after closing all cracks and rendering the air as moist as

possible. Under these conditions sulphur dioxide certainly has a decided value as a disinfectant. It does not, however, penetrate. Carpets must be taken up and removed to the disinfecting plant. Bedding and similar articles and upholstered furniture must also be removed.

As regards the dangers attending the re-occurrence of infectious diseases at hotels, I can only say that we now use the greatest care in the disinfection of the apartments so occupied. In some such instances we have had our men engaged for three days in one suite of apartments. We insist on the walls being repapered in such cases.

Regarding the propagation of cholera by flies, to which Dr. Sternberg referred, I think this aspect of the subject should receive a certain amount of attention. There are some points in connection with the outbreak of cholera last year that are significant in this connection; for example, a large percentage of the cases occurred in those who were engaged in food trades. Out of twelve cases, seven or eight of them, if I remember rightly, were in butchers or in the families of butchers. One or two of the remaining patients were connected with food trades in some way.

Referring to the disinfection of upholstered furniture by wetting with a bichloride solution, this cannot be done, because a great deal of the furniture cannot be disinfected in such a manner. Wetting the surface of plush destroys it, and the same is true with other coverings, so that we must depend, to a large extent, upon the discretion of our disinfectors. Otherwise there would be heavy claims against the department for damages. In specific instances we have destroyed certain pieces of furniture, while in others we have transferred them to the disinfecting stations, where they were thoroughly sprinkled and fumigated.

I am glad that Dr. Bryant compared the facilities of the Health Department to-day with what they were six or seven years ago. There is certainly a striking difference. I think that, as Commissioner of the department, the medical profession and the city owe him a debt that cannot be repaid. His work there cannot be too highly commended. I have been familiar with it for many years, and in many ways with which the medical profession is not familiar the work of the department has been improved and placed on a high standing through his efforts.

Stated Meeting, June 1st, 1893.

OZONE AND ITS USES IN MEDICINE.

By WILLIAM J. MORTON, M.D.

WRITING in the *Asclepiad* in 1890, Dr. W. B. Richardson, of London, the inventor of the ether spray for local anæsthesia, the introducer of peroxide of hydrogen into practical medicine, and in general a brilliant medical pioneer, said:

"In my essay describing the model city, Hygeiopolis, it was suggested that in every town there should be a building like a gas-house in which ozone should be made and stored, and from which it should be dispensed to every street or house at pleasure. This suggestion was made as the final result of observations which had been going on since I first began to work at the subject in 1852.

"It occurred to me from the moment when I first made ozone by Schönbein's method that the value of it from a hygienic point of view was incalculable. To my then young and enthusiastic mind it seemed that in ozone we had a means of stopping all putrefaction, of destroying all causes which produce the great spreading diseases; and although increase of years and greater experience have toned down the enthusiasm, I still believe that one of the most useful fields for investigation remains almost unexplored."

Utopian as the general tenor of the above may have seemed at the time it was written, yet such is the rapid advance in electrical science, in chemical science, in bacteriology, and in the knowledge of the bactericidal properties of ozone, that in reality the day may not be far distant when Richardson's vision may easily mature into reality.

For, electrically, the ozonizer is no longer a plaything; it is a powerful instrument whose output is limited only by the limits of the horse power required to run the machinery which produces the ozone. This transition from an embryo to a mature stage is due to the growth of the induction coil or transformer, now capable of affording from five thousand to one hundred thousand volts, or possibly a million volts. Ozone, moreover, requires no rare chemical for its manufacture; it is merely ordinary oxygen of the air rendered active. With unlimited horse power and unlimited air or oxygen at command, ozone becomes, if desired, a commercial product capable of extensive use.

Chemically, also, progress is made which will work to make ozone available, for ozone has recently been compressed into a small volume and even liquefied. It liquefies about as easily as carbonic dioxide. By compressing a mixture of oxygen and ozone to one hundred and twenty-five atmospheres, Hautefeuille and Chappuis obtained gas in drops of a deep indigo color.

As a bactericide ozone is coming no less favorably to the front.

Recently Ohlmüller and Fröhlich have killed, by means of it, the bacteria of anthrax, typhus, and cholera, as well as anthrax spores, and also the bacillus of pus; and lately I experimented upon the staphylococcus pyogenes aureus, kindly furnished to me by Dr. W. H. Park, and after a few minutes' treatment by ozone the fluid turned from a milky color to limpid, and Dr. Park was unable to procure any cultivation from the treated fluid, showing that ozone really kills the pus germs, ordinarily so tenacious of life.

If, then, up to the present time—that is to say, within a year or two—many physicians and physiologists have held the opinion that ozone is not efficient to purify foul air, to disinfect clothing and dwellings, to sterilize drinking water, and in general to remedy unsanitary conditions, it is because the available apparatus produced but

a small and practically valueless quantity of ozone, and because the method of using it was little understood. Indeed, methods of its use are yet to be investigated.

For many years the writer has been familiar with the ozone produced in considerable quantities from influence electrical machines; has investigated and been astonished at its remarkable chemical properties; has made various attempts to employ these properties medicinally; and now finally, when the facilities are open to all for producing ozone on a large scale, has thought it would not prove uninteresting to collect some of the scattered data and present them as a whole to the members of the Academy, adding some contributions of his own.

But undoubtedly the most important recent contribution toward investing ozone with properties of high value as a purifier of foul air and as a bactericide is contained in an article in a late issue of a report of the German Government (entitled *Arbeiten aus dem kaiserlichen Gesundheitsamte*, vol. viii., first part, 1892), giving in detail the results of experiments made by Dr. Ohlmüller, of the Imperial Board of Health ("Ueber die Einwirkung des Ozons auf Bakterien").

Dr. Ohlmüller in this paper refers to the labors of Wolffhügel ("Ueber den sanitären Werth des atmosphärischen Ozons," *Zeitschrift für Biologie*, vol. xi.). Wolffhügel's experiments confirmed Schönbein's opinion that the ozone in the atmosphere was a disinfecting agent, and that its varying quantities, as observed in dwelling rooms by different observers, were due to the presence in greater or smaller amount of organic impurities in the air. The correctness of this assumption was confirmed by experiment. One hundred and fifty-five litres of air taken from the open showed with the ozonoscope a coloration corresponding to Scale VI. The same quantity of air passed through a eudiometer, into which dust from the street had been blown, showed only a coloration corresponding to Scales III. and IV. This dust incinerated with sodium lime gave a strong ammonia reaction. A sample of the

dust thus treated was exposed to the action of two hundred and sixteen litres of air, and showed with the eudiometer a coloration = V of the ozonoscope. A control experiment with another eudiometer, but without the dust, gave the same scale (V), demonstrating that the organic matter of the dust absorbed the ozone from the air.

Ohlmüller is of opinion that the reason of the many failures in the attempts to ozonize the air of sick-rooms is due to the comparatively small amount of ozone employed, and which consequently was insufficient to destroy the organic matter present.

He also takes the view that the cause of the failure of so many observers (Grossmann, Meyerhausen, Szpilmann, Fischer, Oberdörfer, Sonntagg, Oerum, Wyssokowitsch, Krukowitsch, Lukaschewitsch, and others) and the varying results obtained by them were due to the relatively small quantities of ozone they had at disposal for their experiments.

For this reason it was important to investigate under what conditions ozone acts on bacteria, especially as a favorable opportunity presented itself for obtaining ozone in unlimited quantity.

He found during his researches, under these more favorable conditions, that if the ozone and the bacteria be dry no effect of the ozone on the bacteria was apparent.

With the bacteria dry and the ozone moist, typhus bacilli were destroyed in thirty minutes with forty-six litres of ozonized air = 289.8 milligrammes of ozone.

With ozone and bacteria both moist, typhus bacilli were destroyed in an hour, one hundred and eight litres of ozonized air being consumed.

His experiments with pus showed that after twenty-four hours with a consumption of two litres of ozonized air a minute = 11.5 milligrammes of ozone, the bacilli were killed (staphylococcus pyogenes aureus), as was proved by inoculation of living subjects. Typhus, cholera, and anthrax bacilli placed in distilled and sterilized water were destroyed. Anthrax spores, after passing through

water, 89.9 milligrammes of ozone; anthrax bacilli, 58 milligrammes of ozone; typhus and cholera bacilli, 19.5 and 16.5 milligrammes of ozone.

His experiments with sewage from the river Spree and water containing typhus and cholera bacilli demonstrated that the ozone had a destructive action.

He sums up by stating that the results of his experiments establish with certainty that ozone has a powerful destructive action on bacteria if the water containing them is not too strongly impregnated with lifeless organic matter (*lebloser organischer Substanz*). The result is the same when the lifeless organic matter is first oxidized to a certain degree by ozone.

In connection with cholera, now that ozone is known to be absorbed and again given up from fluids, we must note a valuable contribution entitled "Cholera: its Prevention and Treatment," by Elmer Lee, M.D., of Chicago, and "Hydrogen Peroxide in Contagious Diseases," by Cyrus Edson, M.D., reprint from the *Doctor of Hygiene* for April last.

Both Dr. Lee and Dr. Edson [137] recommend free irrigation of the lower intestine as devised and recommended by Dr. Lee, and the free administration of peroxide of hydrogen, both by the stomach and by intestinal irrigation.

Dr. Edson says: "It is my opinion that this treatment will prove to be *par excellence* the treatment for cholera nostras, dysentery, typhus, and typhoid fever." As oil vehicles may carry as high as twenty-five volumes per cent of ozone, it is obvious that they likewise may be effectually employed in the same diseases.

Fluid Vehicles for Ozone.—The Ozone Company have put upon the market several fluids containing ozone, giving to them the names aquozon, therapol, aerozol, kodozonol, and kreozonol. In spite of these unattractive names these liquids contain a large volume per cent of ozone.

Ordinarily a hundred volumes of water will hold but

half a volume per cent of ozone, but by adding to the water about two per cent and a half of hypophosphite of sodium it will hold about three volumes per cent of ozone.

Aquozon, ozonized water, contains about three volumes per cent of ozone.

Therapol, a bland vegetable oil ozonized, contains about eight volumes per cent of ozone.

Aerozol, a combination of essential oils, the principal one of which is oil of spruce, contains twenty-five volumes per cent of ozone.

Kodozonol is simply ozonized cod-liver oil. It contains about six volumes per cent.

Kreozonol is an ozonized oil of tar. It contains about ten volumes per cent of ozone.

The volume percentage is determined by the amount of ozone set free by various reagents. The fact that aquozon, therapol, and aerozol contain ozone, and contain it in the percentages named, is established by the examination and report of Prof. R. A. Witthaus. That oils absorb ozone in very large quantity and part with it slowly indeed requires no special corroboration, since it has been well known for a long time. The combination is not an oxidation, as one might expect, but the ozone is taken up by the oil as O_3 and yielded up as O_3. In the aquozon preparation the hypophosphite of sodium is not oxidized and converted into a phosphate, but it remains intact, and tests applied show the reactions of both the hypophosphite and the ozone.

Some of the writer's personal experiences and suggestions in regard to the therapeutic uses of ozone follow and may be of interest in advancing its employment.

His attention was first called to the therapeutic effects of ozone in 1884, when, experimentally, he caused a child suffering from severe chronic nasal catarrh to inhale the ozone produced by an influence machine. The acrid discharges, the falling back of secretions into the throat, the altered voice, and the continual sounds of attempts to

clear the passages ceased after six inhalations. A sister of this child was also treated and cured.

Last fall, procuring a large machine from the Ozone Company, I made some further experiments as occasion arose. The machine itself will be described further on.

My first effort was to obviate the irritating properties of the ozonized air delivered from this or any large machine.

It is obvious that the electric effluvium, as it crosses the air space, will affect the nitrogen as well as the oxygen of the air. Thus not only is O_2, common oxygen, converted into O_3, or ozone, but also nitric acid and other noxious nitrogen compounds are formed. Accordingly I used a wash bottle containing a solution of caustic soda or potash to pass the output of the machine through and to absorb the nitric acid. The ozone is now pure. This has added greatly to the respirability of the gas and obviated deleterious effects due to the nitrogen compounds. But even the washed gas, if inhaled when too strong, may irritate the bronchial mucous membrane and produce an asthmatic attack, with suffocative breathing, cough, and final mucous expectoration. As gutta percha is quickly destroyed, as well also other organic substances, I have had attached a goose-necked glass tube ending in a perforated cork. A variety of glass inhaling tubes may be attached to the cork; for the nose, simple straight tubes rounded at one end.

The preparations of ozone and the gas itself, when administered medicinally, part with their ozone within the human economy—just where, we would wish some one to determine. The most hopeful and general view is that the ozone increases the oxyhæmoglobin. This, at least, is conceded when ozone is taken into the lungs in gaseous form; the same may be true of the liquid preparations.

As it does not come within everybody's reach to own a large ozone machine, the importance of these prepara-

tions which hold ozone until it may be liberated within or upon some part of the patient is manifest.

The oil preparations give up their ozone when taken internally, and equally so when applied to external parts, as, for instance, to ulcers, diphtheritic membranes, and wherever a wound dressing is required. It is noted that these oils relieve pain more quickly than if they did not contain ozone.

Another improvement to the commercial machine was to have attached to it means for measuring its output, in order to be able to express dosage in exact terms. This was effected by the aid of a meter to measure the amount of oxygen or air passed through the ozonizing tubes, and collecting the gas to estimate by reagents the number of milligrammes to a litre.

The following table exhibits the measurements of ozone in oxygen and in air produced by the generator of the Ozone Manufacturing Company's manufacture:

Ozonized oxygen,	1 litre	a min.;	quan. of ozone,		79.20 mgm.
"	"	2 litres	"	"	135.60 mgm.
"	"	3 "	"	"	148.08 mgm.
"	"	4 "	"	"	168.00 mgm.
Ozonized air,	1 litre	"	"	"	15.84 mgm.
" (3 litres)	$\frac{1}{10}$ cub. ft.	"	"	"	45.84 mgm.
" (6 litres)	$\frac{2}{10}$ "	"	"	"	70.00 mgm.

$\frac{1}{10}$ cubic foot = about 3 litres.

Nasal Catarrh.—Without making any especial effort to secure cases, and purely as an experiment, I have treated a large number of nasal catarrhal affections by ozone and have been surprised at the good results. Running the machine at a rate which produces about seventy milligrammes per minute of ozonized air, purified from nitric acid by the caustic soda, I cause the patient to blow and cleanse the nose as thoroughly as possible, to take a deep inspiration and hold the breath, allowing the ozonized air to be blown up one nostril and out at the other as long as the breath could be held; the patient is then instructed to

blow the ozone gas out of the nostrils before inspiring, in order to avoid drawing the gas, in this strength irritating, into the lungs. This is repeated about six times to each nostril. The first feeling is one of intense dryness of the mucous membranes; then follows possibly a slight watery mucous discharge lasting for a few hours, and then next and subsequently the patient experiences great relief. This treatment was pursued thrice weekly, and five to ten treatments were sufficient to effect results. After treating ten cases successfully, and not caring to follow up nasal catarrh any further, I sent an eleventh case, a boy of twelve, who had suffered for two years, and who was so remarkably improved in five treatments as to attract much attention from his family, to my friend Dr. Clarence C. Rice to examine, with the suggestion to follow the matter up. The boy was a confirmed mouth-breather, his nose was thickened and red, his speech altered, and a continuous acrid discharge caused his upper lip to be constantly excoriated. He received ozone treatment two or three times weekly during about three weeks. The acrid discharge ceased, the lip healed, and he breathed well through the nose. Dr. Rice's examination confirmed these statements as to recovery and called attention to a rather marked adenoid enlargement in the vault of the pharynx. Following this case, Dr. Rice and I treated eight more cases on the indications and by the method above outlined. They were cases of atrophic rhinitis and dry pharynx. They all improved.

Delay in publishing this paper enables me to insert Dr. Rice's description of these cases, embodied in a communication read before the American Laryngological Association a few weeks after this paper was read at the New York Academy of Medicine:

"Three of my cases were advanced to an extreme condition of atrophic change, so familiar to all of you. In two the turbinates had almost entirely disappeared. The septum was ulcerated, and the secretions were collected in thick scabs. Of the two cases I sent to Dr. Morton's

office, one was far advanced in atrophic change and the second was but moderate in degree. All of the cases were treated with the ozone gas from five to eight minutes. In Dr. Morton's cases the gas was passed into the nostrils three times—that is, while the patient was holding the breath for three different periods. The immediate effect of the ozone gas was alike in kind in all the cases, but differed in degree according to the volume of the gas employed, and probably according to the sensitiveness of the nostrils. It produced a mild smarting of the nasal mucous membrane for several hours. It increased the secretions of mucus very markedly for eight or ten hours. The congestion of the mucous membrane of the nose was also exaggerated. In some of the cases, where the volume of gas was large, the patients complained of headache, which, however, had disappeared by the following day. It was almost the universal testimony of these patients that the nasal pharyngeal mucous membrane was less dry, and that the tenacious secretions were more easily got rid of. For the first twenty-four hours after the ozone was used the patient had the symptoms of a cold in the head, but after that the nose felt unusually clear and the mucous membrane more comfortable than before the application of ozone.

"The milder cases of atrophic catarrh were very quickly benefited, and remained improved for several days after the inhaling of the ozone, but most of them relapsed somewhat. The benefit was again obtained, however, by the further treatment with ozone. Whether permanent moistening of the mucous membrane and decrease of secretions can be obtained by the use of ozone can be told only after a longer trial. In the more severe cases the benefit was not obtained so quickly. In all the cases we instructed the patient to discontinue the use of all sprays and douches while we were using the ozone. In a case of ozæna treated by this method the deodorizing power of the ozone seemed to be as marked as is obtained by the use of peroxide of hydrogen. The odor from the nostrils was very fœtid, but disappeared altogether after the second inhalation of ozone gas." [1]

From further experience I am satisfied that results are equally good in hypertrophic catarrh, and also satisfied that a large volume of ozonized air—from forty-five to

[1] New York Medical Journal, August 19th, 1893.

seventy milligrammes a minute—is essential to success in nasal cases. It is a great mistake to suppose that the mere smell of ozone will cure rhinitis.

Bronchitis.—Reducing the output of the machine to a point affording about one milligramme per minute, I have caused ozonized air, purified from nitric acid as before, to be inhaled in bronchitis, both acute and chronic, with great relief to the patients.

I cite but one case, since I simply intend my paper to be suggestive:

CASE I.—December 15th, 1891. Lieutenant F. P. F., aged thirty-six years. Between twenty and twenty-five had several pulmonary hæmorrhages. Three years ago, after exposure in the Northwest, had a severe attack of what was carefully diagnosticated to be capillary bronchitis; lost forty pounds in weight in ten days, had severe cough and other characteristic symptoms. Has had a persistent and severe cough ever since; sputa dark yellow, blood-flaked, and adhesive. Has severe and constant pain in the left side alongside the sternum on a level with the fourth rib. Had been treated variously and with no relief for the chronic bronchitis which had supervened upon the cessation of the acute attack of capillary bronchitis. Physical examination revealed mainly the usual bronchial râles. He was treated four times—the 15th, the 19th, the 23d, and the 27th of December. The last entry in the note-book is: All the symptoms ceased: no cough at all.

Unfortunately for scientific accuracy, the treatment was by statical electricity as well as by ozone inhalations. Since the case is somewhat unique, I read the patient's own letter, written January 25th, about a month later. He says:

"BEDLOE'S ISLAND, NEW YORK HARBOR,
"January 25th.

"MY DEAR DOCTOR:—You have yourself to blame that you have not seen me lately, for since the last treatment I have not coughed once. I know that this reads like the remarks about patent medicines, where two bottles make the patients five years younger; but I never expected to

be free from that infernal cough, and having to get up in the night and shut myself up in a closet to avoid waking the rest of the family up. When I catch a little cold now it goes off again and I never have the faintest pain in my chest. This is pretty good after three years of constant trouble that even the climate of California failed in the least to alleviate. I am coming up some day soon to show myself. Sincerely yours, F. P. F."

Phthisis.—I will likewise offer but one case of phthisis:

Miss R., aged twenty-eight years. Referred to me by Dr. Frederick A. Burrall, of this city, with history of active pulmonary tuberculosis. Has had several attacks of hæmorrhage; has dulness under the right clavicle and dry pleurisy; has harassing cough, frequent slight hæmorrhages; cannot retain her morning meal, and had reached a point where she was losing weight and her food was not assimilated. Duration of disease about two years. The noon temperature was usually about 99.5°; after treatment it was lowered to about 99°, though these relations varied from time to time. The patient received three treatments a week only. Since the third she has never failed to keep down her breakfast. She has received about twelve treatments and has gained six pounds in weight. Both statical electricity and ozone were administered. Her physician reports: "The cough has much diminished, the nausea quickly disappeared, patient has gained six pounds and has evidently assimilated food and remedies. I notice the same improved pharyngeal hue from pale to ruddy which I noticed in the previous case [under the same treatment]. Her unfavorable symptoms are all generally ameliorated."

This was a year ago, and from reports received last summer I understand the patient retained the improvement gained.

Bactericidal.—Dr. William Hallock Park, of this city, connected with the laboratory of the Vanderbilt Clinic, College of Physicians and Surgeons, kindly consented to join me in some experiments upon killing bacteria. We have not yet completed these experiments, owing to unforeseen delays, but I submit a preliminary report from Dr. Park which is very satisfactory, so far as relates to

the action of ozone when it is passed through fluids holding bacteria:

March 26th, 1893.

DEAR DR. MORTON:—The cultures made on the gelatin plates proved the preliminary report to be correct.

A summary of the experiments is as follows:

A considerable amount of a twenty-four hours' growth on agar of the staphylococcus pyogenes aureus was scraped off and suspended in five cubic centimetres of sterilized filtered Croton water.

A culture of Klebs-Löffler or diphtheria bacilli was treated in a similar way.

Small pieces of sterilized absorbent gauze were saturated with these cultures thus suspended in water and the pieces placed on a sterilized platinum wire net. This was placed in a wide tube through which a fifth of a cubic foot of ozonized air passed per minute. The bacteria were exposed different lengths of time—the staphylococci from five to thirty-five minutes and the diphtheria bacilli fifteen to forty-five minutes. On removal the gauze was still moist.

Cultures of the staphylococci were made on agar and in gelatin. Those exposed for five minutes gave about as many colonies as the control, and those exposed the longest (thirty-five minutes) about half as many. The cultures of the Löffler bacilli were made on solidified blood serum.

Those exposed fifteen minutes showed some diminution in the number of colonies on the cultures, and those forty-five minutes still more, but still there were very abundant colonies.

The results showed that the staphylococci and the diphtheria bacilli when held on a moist piece of gauze are not greatly injured by exposure of thirty to forty-five minutes to a current of ozonized air.

The second experiment was to pass the ozonized air into the test tubes containing a watery suspension of the staphylococci. The air was passed into the mixture for

five, ten, and fifteen minutes. The cloudy fluid was seen to quickly become clearer under the action of the ozone.

Cultures were absolutely sterile.

The result therefore showed that in the shortest trial (five minutes) all the staphylococci were killed by the action of the ozonized air dissolved by the fluid.

If you would like any fuller report let me know. I think it would be interesting to try some further experiments in passing the ozone into fluids. This part seemed very successful.

Very sincerely,

[Signed] WILLIAM HALLOCK PARK.

Again, in the way of suggestion, I can see no good reason why gaseous ozonized air or oxygen may not be administered per rectum in cholera and typhus and typhoid fevers, just as in the familiar rectal gaseous administrations of sulphuretted hydrogen for phthisis, though I am not aware that this method has been suggested or utilized in any disease.

There is another perhaps unimportant but novel point to which I may call attention. Taking, for example, iodide of potassium, free iodine is produced in its nascent state when ozone comes into contact with it.

Supposing that ulcerations, skin diseases, the nasal and throat cavities, and other mucous surfaces and numerous conditions which will occur to all, be treated to a solution of iodide of potassium or any similar solution acted upon by ozone. The iodine or other liberated chemical acts in its nascent state upon the tissue.

In this manner the nose and throat might be first treated to a medicated spray and then to ozonized air, and the result be a new compound applied topically and nascently.

I have dwelt perhaps a little at length upon some of the therapeutic possibilities of ozone. There can be no doubt, it seems to me, that, now that ozone may be generated on a large scale, its therapeutic properties should be tested on an equally large scale in hospitals and definite knowledge be obtained. More frequent and better op-

portunities present themselves in hospitals for testing the therapeutic value of ozone than are afforded in private practice. Ozone has too long been a sort of medical chimera; it is time that some one dispelled the illusions and laid bare the facts. One truth is clear: it is an oxidizing agency scarcely if at all inferior to nascent oxygen, while it possesses the advantage over nascent oxygen that it exists in gaseous form and may be disseminated as a gas into parts where nascent oxygen cannot be utilized. Nascent oxygen cannot purify the air of a hospital ward nor can it enter the lungs; its action is strictly localized to the very spot of its birth.

If we turn to the prophylactic and hygienic properties of ozone we must be struck with admiration.

Ozone attacks and renders innocuous organic matters such as smoke, sulphur compounds, and the great variety of organic matters which contaminate the air we breathe. Or, if we put it another way, these organic matters destroy or absorb ozone. Think of the quantity of ozone that would be destroyed in hospital wards, theatres, public halls, and schools.

Again, ozone sterilizes and purifies water; the most impure water can at once be rendered sweet-smelling, free from organic impurities, and free from pathogenic germs by passing ozone through it.

The purification of the water for drinking purposes of the river Spree is one of the prime objects of the investigations by the Imperial Board of Health of Germany, from whose report I have given some data.

It has been stated to me that our own Croton Water Board has appropriated a large sum of money to endeavor to secure the purification of Croton water by ozone. A gentleman interested stated that a first idea was to construct a great screen of platinum wire, through which the water should pass, while at the same time the screen formed an electrode to the current from great dynamo machines at work upon the banks of the river. One objection to this plan was that the sand would cut and wear

away the wire—a matter of no small importance, since its value was estimated at twenty-eight thousand dollars.

In sufficient strength and quantity and in the presence of moisture ozone is, as I have abundantly shown, a disinfectant. When we recall the ever-latent danger of an invasion of cholera, and recall equally the exacting demands of a strict quarantine disinfection, we cannot but feel that ozone, as now producible in quantity, may most properly form the subject of investigation on the part of our own Board of Health.

The problems which beset the utilization of ozone as a therapeutic agent are manifold—more difficult of solution, perhaps, than those which surround its introduction into the public uses outlined by Dr. Richardson in the opening sentence of this paper; but when experimentation and observation, more accurate and of wider scope than any now at command, shall be available, I have no doubt that ozone, like many another idea that has floated upon the medical horizon, will assume an important relation to the health of the individual and of the community.

Historical.—It is to the labors of Schönbein (1839) that we owe the identification of ozone with oxygen, or rather nascent, electrified, or contracted oxygen, for, although unnamed, it was not unknown. Its peculiar "sulphurous" odor, perceptible after electrical discharge during heavy thunderstorms, had been perceived even in remote ages, as is evidenced by a reference made by Mohr[1] to some verses in the "Iliad" (viii., 135; xiv., 307) and in the "Odyssey" (xii., 417; xiv., 307).

Other authors who make mention of the peculiar odor accompanying discharges of lightning: Virgil, "Æneid," ii., 691–698. Seneca, "Quæstionum naturalium," ii., 21–53. Lucretius, "De rerum natura," vi., 221. Lucanus, "Pharsalia," vii., 221. Shakspere, "Cymbeline," act v., scene 4; "King Lear," act iii., scene 2; "Tempest," act i., scene 2; "Pericles," act iii., scene 1, and other passages.

Benjamin Franklin (1749) ascribed the odor accom-

panying the working of an electrical machine and a discharge of lightning to decomposition produced by electricity.[133]

The odor of ozone was also known to those experimenting with frictional electricity, but Martinus van Marum[2] was the first who made any investigations as to its nature. He experimented at Haarlem in 1785 with the discharge of the electric spark through the atmosphere and also pure oxygen. He showed that when the electric spark is allowed to act for some time upon "dephlogisticated air" or oxygen, contained in a closed vessel over mercury, a considerable diminution of the gas took place, the mercury was oxidized, and the oxygen acquired an intense odor.

Cruikshank[3] also observed in 1801 that a peculiar odor was perceptible during the electrolytic decomposition of water, which odor we now know arises from ozone.

From this period until 1839 nothing was observed in regard to the nature of ozone. On the 13th of March of this year Schönbein[4] communicated to the Naturalists' Society at Bâle his experiments and observations of the characteristic phosphorus-like odor apparent during the electrolysis of water and the discharge of frictional electricity through the atmosphere. To the new substance he gave the name of ozone, from ὀζων, present participle of ὄζω (I smell). He considered it a substance similar to chlorine or bromine. On the 3d and 17th of April and on the 15th of May, 1844,[5] he again laid before the same society his researches, in a communication describing his discovery that ozone was produced during the slow oxidation of phosphorus in moist atmospheric air. He had, however, changed his views regarding it, believing it now to be a component part of nitrogen, and that this latter element was a combination of ozone and hydrogen.

The true nature of ozone was long a moot point and engrossed the attention of a number of investigators: De la Rive,[6] Marignac,[6] Frémy,[7] Becquerel,[7] Andrews,[8] Tait,[8] Von Babo,[9] Fischer,[10] Baumert,[11] Soret,[12] and others.

Odling [122] in 1861 suggested that ozone was a compound of oxygen with oxygen, the combination resulting in a contraction. He also conjectured that this contraction might proceed from a condensation of three volumes of oxygen into two volumes.

It was not until the year 1863 that a really precise definition of the constitution of ozone was promulgated. Soret [13] was the first to pronounce that ozone was really oxygen condensed to two-thirds of its volume. His view was at first disputed by some investigators, but eventually confirmed by experiments conducted by himself and others. This opinion as to the constitution of ozone is, up to the present day, the one generally accepted. According to this acceptation the ozone molecule consists of three atoms of oxygen, whereas the ordinary oxygen molecule contains but two atoms:

Ozone.

Ordinary Oxygen.

O=O

Baumann [13] styles the combinations of oxygen as follows:

$(O)_2$ Ordinary inactive oxygen,

$(O)_3$ Ozone,

$(O)_1$ Active oxygen,

the latter being the most powerful oxidizing agent known. It combines with ordinary oxygen to form ozone.

Production of Ozone.—The great source of ozone is found in the vast operations of Nature. Schönbein, on his discovery of this element, called attention to the fact that the peculiar "sulphurous" or "phosphorous" odor perceptible in some localities, attendant upon a stroke of lightning, derived its origin from ozone. Andrews and Tait confirmed his views. It is an acknowledged fact that the silent discharge or interchange of opposite electricities from clouds to earth is a factor in the production of ozone; the intervening space being filled with the atmosphere, all the conditions are present necessary to the generation of this gas. The correctness of this theory

is supported by the observations of Houzeau and others. Houzeau [14] found that during thunderstorms the air was particularly rich in ozone.

An examination of the atmosphere with the spectroscope reveals the presence of dark absorption lines in the yellow and red parts of the spectrum, always in the same place, proving the existence of ozone in the atmosphere. It has been proved that the color of ozone in a state of great density is blue; it therefore absorbs yellow and red light.[33]

Meissner [15] was of opinion that ozone took an active part in the formation of clouds.

Ozone is also formed by the evaporation constantly taking place on the earth's surface; in fact, Von Gorup-Besanez [16] and Belluci [17] observed that it was developed whenever evaporation of water was in rapid process, and the more energetic the process of evaporation the greater was the amount of ozone. The quantity was also conditioned by the greater or less proportion of salts held in solution by the water. These investigators also found the air strongly ozonized in the neighborhood of waterfalls, irrigation systems, drying houses, near the sea, etc.

Scoutetten,[18] Brame,[19] and Kosmann [20] showed that plants exhale ozone, together with the oxygen which they give out during the daytime.

According to Ebermayer,[21] ozone appears to be present in increased quantity in the vicinity of forests, especially those where trees of the pinus group predominate.

There are a great number of processes by means of which ozone may be produced mechanically, chemically, or by electricity.

Mechanically.—When air is forced through a narrow opening it is found to contain ozone.[22]

The nobler metals in a finely divided state, such as gold and platinum, absorb a certain amount of oxygen and retain it in a condensed form, but yield it up to oxidizable bodies, with which ordinary oxygen shows no reaction;

iodide of potassium and starch showing an ozone reaction when brought into contact with black platinum, etc.[23]

Chemically.—Oxygen in contact with water or saline solutions in process of evaporation forms ozone.[24]

Whenever oxygen enters into chemical combination it exists in the nascent state, with the accompanying formation of ozone. The moment that the oxygen is set free some of its atoms, not as yet aggregated to molecules, unite with the already formed oxygen molecules and produce ozone.[25]

Schönbein[26] and Kingzett[27] noted the existence of ozone upon heating certain metallic oxides.

Phosphorus, oxidized at the ordinary temperature of the air,[28] produces ozone, and the same result is obtained if a current of air is passed slowly through a tube filled with moist pieces of phosphorus. This method was for a long time the general one in use by most experimenters until the invention of the apparatus by means of which the gas is obtained by the electric process.

Active oxygen is generated during every process where draught and low temperature promote its production and preservation. A strong current of cold air passing through the small flame of a Bunsen burner or a hydrogen flame is ozonized, and may be recognized by its peculiar odor.[29]

Deville[123] produced ozone by means of a hot and cold tube. Oxygen heated to 1,400° and immediately cooled is converted into ozone. Similar conditions are present in ozone tubes, where the luminous discharge raises the temperature of the gas, which is then abruptly cooled by its contact with the rest of the gaseous mass in circulation.

Electrically.—Cruikshank[30] and later on Schönbein[30] showed that during the electrolysis of water the characteristic odor of ozone is perceptible.

The generation of ozone by the static electrical machine[31] is a fact well known to those interested in electric

science. Both during the spark and brush discharge the odor of ozone is unmistakable.

The method now most extensively employed for the manufacture of ozone is that in which the silent discharge [32] or electrical effluvium is allowed to traverse a narrow dielectric space between two conductors, and, acting upon a stream of oxygen or atmospheric air, generates ozone in large quantities.

Ozone is also evolved by the electrolysis [124] of water, but only in small quantity.

According to Brodie, ozone may be obtained from carbon dioxide by passing this gas rapidly through an induction tube and subjecting it to electric action. By this means he succeeded in converting seventy-five per cent, and even eighty-five per cent, of the eliminated oxygen into ozone.

Chemical.—Ozone is a colorless gas with an intense characteristic odor, reminding one, in a small degree, of chlorine and nitrous acid. Air containing 1 to 50,000 parts of ozone possesses still this peculiar odor.[33] According to Soret the density of ozone is 1.658.[34] The active oxygen generated by treating barium peroxide with concentrated sulphuric acid has quite a different, nauseating odor which causes a choking sensation in the throat.[35] Hitherto ozone has only been obtainable in a diluted state, although both Chappuis [36] and Olzewski [37] claim to have liquefied it. Chappuis succeeded in accomplishing this at a temperature of —105°, with a pressure of 125 atmospheres. Olzewski achieved its liquefaction in sufficient quantity to determine its boiling point, which he states is —159°. The liquid was dark blue in color and nearly opaque in a layer a sixteenth of an inch thick.

Prof. James Dewar, in his experiments with liquid oxygen, obtained by electric stimulation of the liquefied gas twenty per cent of ozone, which was of a very dark-blue color—as dark as concentrated indigo. The ozone is exceedingly unstable—to such a degree, indeed, that a beam of light caused it to explode on one occasion. The

study of ozone in such concentration requires great delicacy of manipulation.

Scoutetten[38] is authority for the statement that ozone is destroyed at a temperature of 250° C. He passed ozone through a tube heated up to this point.

The solubility of ozone in water was a long-disputed point,[39] but all doubt thereon has been removed by the labors and investigations of Engler,[40] Nasse,[40] Houzeau,[41] and particularly more lately Carius,[42] Schöne,[43] and Leeds.[44]

E. Schöne found that ozone obtained by means of the silent electric discharge was soluble in water to the extent of 8.81 cubic centimetres to the litre.

This question has received further confirmation by J. C. Dittrich, a German chemist residing in New York, who has long been experimenting with ozone. He has succeeded in forming a stable compound of this gas with water by the addition of a very small percentage of hypophosphites.

The statement that the action[125] of ozone on water produced peroxide of hydrogen has been negatived by the experiments of Nasse and Engler,[126] Carius,[127] and Schöne.[128]

Hydrogen peroxide[130] is decomposed by ozone, ordinary oxygen being liberated and water formed. Hydrogen peroxide and ozone are essentially different. Hydrogen peroxide exists in liquid form at ordinary temperature and pressure. Ozone is a gas and is only soluble in water to a small extent, whereas hydrogen peroxide is readily soluble in water.

Statements have been made that oxygen passed through a solution of hydrogen peroxide becomes charged with ozone. Experiments lately made in this city, in which every precaution was taken to exclude all possibility of error, established with absolute certainty that ozone was not produced by any such process as referred to above. No ozone could be detected even with the most delicate known tests. The enumeration of all the distinctive

properties of O_3 and H_2O_2 do not lie within the scope of this paper. The reagents employed for their determination are given by Baumann and Gawalowski respectively.

Ozone oxidizes both mercury and silver when moisture is present, but if the metals and the ozone are perfectly dry no oxidation takes place. If the experiment be sufficiently prolonged, the absorption of the gas takes place, even to the complete disappearance of the volume of oxygen employed. Iodide of potassium in solution is immediately decomposed, assuming a well-marked yellow color. On the addition of a solution of starch the usual iodine reaction takes place and the solution turns an intense blue.[44]

Organic coloring matters exposed to the action of ozone are rapidly destroyed. Chlorine, bromine, and iodine combine with ozone when water is present, forming chloric, bromic, and iodic acids. Ozonized atmospheric air in the presence of lime water produces nitrate of lime in appreciable quantity.[45]

Ozone precipitates peroxide of lead in an alkaline solution of lead or its acetate.[44] It rapidly decomposes all the salts of manganous oxide,[45] either in the solid state or in solution, forming the peroxide of this metal.[45]

A solution of ferrocyanide of potassium is transformed by ozone into the red ferricyanide.[45] Uric acid is oxidized by ozone to alloxanic acid and urea; the albuminates and sugar are also destroyed by its action.[45]

A great number of metallic sulphides are rapidly transformed by ozone into sulphates, particularly the sulphides of iron, lead, copper, and antimony.[45]

Baumann [132] observed that ferrous oxide enters into direct combination with ozonized oxygen, without forming active oxygen or liberating it.

Sulphur and selenium undergo no change when submitted to the action of ozone.[46]

Phosphorus acted upon by ozone forms phosphorous acid, which is afterward converted into phosphoric acid.[47]

Pyrogallic and gallic acids, dry or moist, are at first turned brown, and then decomposed into carbonic acid and water.[46]

Fluorine appears to exert some action on ozone. It has been shown by Moissan that fluorine decomposes water, forming hydrofluoric acid, and liberating oxygen in the form of ozone. If a few drops of water be placed on the floor of the experimenting tube and fluorine gas be passed in, a dark fog is seen surrounding each drop, which presently clears and resolves itself into the characteristic blue vapor, apparently more than an inch in thickness, and which is found to be that most interesting condensed form of oxygen—ozone—in a state of great density.[48]

Nitroglycerin, gun-cotton, iodide of nitrogen, and boride of nitrogen explode in ozone.[47]

Of paramount importance in the economy of Nature is the action of ozone in its relation to ammonia. It forms, as shown by Carius, nitrous and nitric acids, and, when water is present, a small quantity of hydrogen peroxide. This reaction is essentially important, as the atmosphere is thereby cleared of its ammoniacal constituents, which are disseminated in the air from the processes of decay and putrefaction.[47]

Ozone is rapidly absorbed by a large number of vegetable and animal substances, such as albumin, casein, fibrin, and the blood.[50] On passing ozone through blood it turns dark at first and then black; on continuing the process the blood forms a clear fluid, containing no albumin, a quantity of gas being given off during the operation.[51]

The bleaching properties of ozone are well known. Lawn bleaching is due to the presence of ozone in the atmosphere. As ozone does not affect the most delicate fibres, it is probable that it will in the near future find employment in industrial bleaching processes. Houzeau states that ozone possesses thirty to forty times the bleaching power of chlorine.[52]

The action of ozone as an oxidizer, when in moist condition, is of such an energetic nature that it is difficult to

construct apparatus the materials of which are not partly destroyed by it. Hence cork, india rubber, wood, paper, and most of the metals are not satisfactory substances for use in the construction of such parts of apparatus as may come into immediate contact with this gas.[53]

Ozone is believed to act as an oxidizer just as chlorine and peroxide of hydrogen do. Chlorine seizes upon the H_2 of H_2O and sets free nascent oxygen; peroxide of hydrogen gives up one atom of unstable oxygen, and again we have nascent oxygen; O_3, or ozone, gives up one atom of oxygen, and thus again we have nascent oxygen. This at least seems to be a received view, but it appears to me that there is some evidence to show that O_3 oxidizes, not by parting with one atom of oxygen, but by uniting as O_3, and thus forming its own special reactions with organic compounds, differing from simple oxidations by nascent oxygen.

Oil of turpentine, however, according to some investigators, absorbs the three atoms of oxygen of which ozone is constituted.[47]

Antiseptic Properties.—The antiseptic properties of ozone are well known. Schönbein showed that $\frac{1}{3240000}$ of ozone in the air was sufficient to prevent the odor of decay in a vessel of sixty litres in which had been placed for one minute one hundred and twenty grammes of decaying, stinking meat.[54]

Wood and Richardson passed ozonized oxygen through blood in course of decay, with the result that the blood entirely lost its offensive odor, and even regained, to a certain extent, its original odor of fresh blood.[55]

Fox,[55] who also experimented with ozone, passing it through putrid matter, concluded that the bacteria in this matter were destroyed.

Scoutetten[56] experimented in the same direction. He placed pieces of meat in vessels containing oxygen and also in vessels containing ordinary air, and found that putrefaction manifested itself most actively in the vessels

filled with oxygen. He then tried the action of ozone on putrid meat and other decayed matter.

He placed a piece of meat in an advanced state of decomposition, weighing two hundred and seventy grammes, giving out an insupportable odor, in a vessel of five litres capacity filled with ozonized air; in one minute the meat was completely disinfected.

He repeated the experiment with three hundred grammes of horse manure emitting a strong ammoniacal odor, which odor disappeared immediately.

Stagnant water poured by the investigator into a vessel containing ozonized air and shaken lost its disagreeable odor immediately.

Experiments on a larger scale were conducted in a room of about eleven hundred metres cubical contents. Two heaps of horse manure were placed about ten metres apart in this room. The manure was in an advanced state of putrefaction and emitted a strong ammoniacal odor. It was allowed to remain in the room for forty-eight hours. When the room was fully infected the contents of four vessels, each of six and eight litres, were poured out. The ammoniacal odor diminished perceptibly, but did not entirely disappear, the manure still continuing to emit its disagreeable smell.

The experiment was continued next day, the manure being, however, removed. The same quantity of ozonized air was allowed to flow over the spots where the manure had been lying; the ammoniacal odor disappeared immediately—so quickly, indeed, as to call forth the astonishment of the bystanders.

Fox,[57] Geissler, Stein,[58] and Chappuis[59] maintained that ozone was not only a deodorizer, but a disinfectant and a germicide.

Experiments have been made with ozone for the preservation of meat, and a patent is in existence covering this process.

The assumption that ozone, owing to its powerful oxidizing properties, would destroy bacteria, has re-

ceived the support of many investigators. On the other hand, many have expressed their views on this point in a negative sense. It must be accepted that upon passing ozone through water containing organic matter and living germs the water is completely sterilized and the bacteria are destroyed. Although there has long been uncertainty as to whether ozone had a destructive action on pathogenic bacteria—viz., those producing typhus, cholera, anthrax, etc.—this question is at length solved in a manner favorable to ozone. It is now merely a question of method and of quantity of ozone.

According to Schönbein,[61] Clement,[61] Richardson,[61] Boillot,[60] and Scoutetten,[67] ozone destroys miasmatic emanations. Their experiments establish that the disappearance of infection and putrid effluvia is effected by ozone when it is present in sufficient quantity.

Pasteur says: "It seems, after all, but stating a truism to say that (extremes of temperature, of course, excepted) it is active oxygen which must be regarded as the greatest known enemy to bacterial life, whether from the action of chlorine, nitric acid, ozone, or H_2O_2. It also follows that these bodies are the best disinfectants." [140]

In Dujardin-Beaumetz's [62] "Dictionary of Therapeutics" appears the following:

"Ozonized air is a deodorizer and antiseptic. It arrests or prevents the putrefaction of vegetable or animal matters and removes all bad odors resulting from decomposition of organic substances. Provided that it be highly charged with ozone, the air even becomes a powerful bactericide. But it is with this gas as with other disinfectants—it is a microbicide only in the condition of being so abundant in the atmosphere that the latter would become a deleterious and irrespirable medium. In a quantity that can be tolerated by the respiratory organs, ozone possesses only disinfectant properties and no longer germicide ones."

The *vexata quæstio* has lately approached much nearer a

demonstration by the labors of Dr. Ohlmüller,[63] referred to above.

Dr. Boutens,[64] of St. Raphael, in a communication presented at a meeting of the Société pour l'Avancement des Sciences, Marseilles, September 23d, 1891, states that ozone is a powerful germicide in tuberculosis.

Dr. Emil Schnee, of Carlsbad, subjected syphilis bacilli and anthrax bacilli cultures to the action of ozone at blood heat, passed through a special disinfecting apparatus. The cultures were placed on small porcelain saucers. The syphilis cultures were destroyed in thirty seconds. The anthrax bacilli, however, withstood the action of the ozone for half an hour. The same results were obtained even when the cultures were enclosed in thin tissue paper.

Oberdörfer's[65] experiments showed that the action of ozone was prevention of the development of bacteria, and even their destruction, so that not only prodigiosus and cholera cultures, but even the spores of anthrax, were killed.

Dr. Wyssokowitsch,[66] of Charkow, convinced himself by a series of experiments that ozone really had a considerable influence on the growth of bacteria. Even small quantities of ozone (0.5 to 1.5 milligrammes in one hundred cubic feet of air) cause a notable difference in their development.

M. Herman, after a series of experiments on the bactericidal value of ozone, is of opinion that ozone is a powerful oxidizer, and as such a potent microbicide in certain conditions, but that it is never present in the atmosphere in quantity sufficient to play the rôle of an antiseptic. He also states that the favorable therapeutic results obtained in certain infectious maladies, particularly tuberculosis, by inhalations of ozone cannot be explained by the antiseptic action of this gas; in fact, ozonized air in quantity sufficient to be a bactericide would be irrespirable.[134]

Many observers have turned their attention to the dis-

infecting rôle that ozone plays in the economy of Nature, and have maintained that it is due to ozone alone that the multiplication of germs of epidemic diseases is prevented. They claim that the spread of epidemic diseases is caused by the absence of or a diminution of the normal quantity of ozone in the atmosphere, and is coincident with the appearance of cholera, etc. Others, however, explain that the absence of ozone is due to the increased number of germs which absorb the ozone, arguing that their opponents mistake the cause for the effect.

One of the latest investigators in this particular direction, M. D. A. van Bastelaer,[61] *membre titulaire*, Académie royale de Belgique, established a series of observing stations throughout Belgium for collecting ozonometric data. He says that some observers contend that the germicidal action of ozone in the atmosphere would require a proportion of ozone superior to that ordinarily present in the air. That is undeniable, if it were a matter of killing the germs immediately; but the fact must not be lost sight of that the oxidizing action of atmospheric ozone is energetic though slow, and that with time it destroys all organic matters in the atmosphere.

Moffat,[67] during the cholera epidemics of 1854 and 1866 in England, made many ozonometric observations which apparently confirmed the coincidence of a diminution or absence of ozone in the air and the appearance of the epidemic.

Cook[68] made similar observations at a series of stations at Bombay in 1866.

Smallwood[69] also noted the fact of a decrease in the quantity of atmospheric ozone during an outbreak of cholera in Canada, and Bœckel[70] believed that similar conditions existed during the cholera epidemic in Strassburg in 1854. Many other observers in different parts of Europe noted a similar coincidence.

One of the latest reports is from Dr. Onimus,[71] who publishes tables of his personal observations made at the Military Hospital in 1883. These tables show an absolute

and direct relation between the ozonometric conditions and the intensity of the epidemic.

Physiology of Ozone.—The physiology of ozone appears to be as yet incomplete. Its action may be explained and summed up in the fact that the oxygen in the blood exists in the condition of ozone, and this property is so characteristic that Schönbein and His[72] proposed giving to the red corpuscles of the blood the name of ozonophores (ozone-carriers). It has been held that the ozone carried by the red corpuscles of the blood alone prevents its coagulation in the veins. The proof of this theory seems to be established by the fact that by passing ozone through coagulated blood it returns to the fluid state.

According to Dr. Otto Ringk[73] the coloring matter of the red corpuscles (hæmatin) contains seven per cent of iron. This iron takes up the ozone with avidity (iron having the property of absorbing ozone without decomposing it) and yields it up, as required, to destroy all deleterious matter which may have found its way into the circulation or tissues.

Dr. J. Aulde[142] has stated that protoplasm has the property of absorbing and storing up oxygen which it receives from the hæmoglobin of the blood, the lymph being the intermediary of transfer. The oxygen thus absorbed is afterward utilized as mechanical energy. It is possible that the protoplasm may, as occasion demands, yield the oxygen again to the intercellular fluid. It also dissociates the oxygen molecule, forming ozone in the organism, where its powerful oxidizing properties are called into play, enabling the protoplasmic cells to eliminate the waste products and continue their reconstructive processes.

M. Girard[76] read a paper before the Academy of Medicine, Paris, at its sitting August 11th, 1891, relating to the action of ozone on the blood. He states:

1. Ozone transforms albumin into fibrin and the hæmoglobin into oxyhæmoglobin.

2. Its prolonged action in the nascent state on this

fibrin is to redissolve it and render it incoagulable by acids or metallic salts.

3. Ozone is the agent in the transformation of the blood into fibrin, and the effects of the ozonization are produced by the iron contained in the globules, and which under the influence of primary ozonization acquires the property of ozonizing ordinary oxygen.

M. D'Arsonval[76] is of opinion that ozone administered by inhalation diminishes the respiratory capacity of the blood.

Prof. Wolfberg,[77] Strassburg, proved that ozone had a remarkable influence on the elimination of carbonic acid from the blood.

W. Jaworski[135] proved by experiment on the behavior of carbonic acid, oxygen, and ozone in the human stomach, that of these three ozone increased most the quantity of gastric juice secreted.

Dr. Ringk[79] deduces from his experiments that the internal administration of ozone ozonizes by means of its oxidizing properties the contents of the stomach, which then, in a condition antagonistic to the development of bacteria, pass on to the intestines, where, on the one hand, the action of the ozone allays any inflammation of the mucous membrane that may exist, and on the other hand establishes conditions unfavorable to the growth of bacilli.

He also was of opinion that it manifested its effect in a twofold manner: first, by being yielded up to the contents of the stomach prior to their passage into the intestinal tract for further digestion, consequently converting the contents of the stomach into ozone-carriers, and, further, by diffusion, finding its way into the blood.

Binz[80] showed that ozonized air acted qualitatively the same as nitrous oxide, but differently in different individuals. His experiments were made on the living human organism. Sleep was produced by inhalations of ozone, which, however, did not continue longer than twelve seconds after the discontinuance of the administra-

tion of the gas. In some cases there was vomiting, in others only an inclination to vomit.

Dr. Ireland [143] states that the action of ozonized air on animals is as follows: 1. It accelerates the respiration and, it may be inferred, the circulation. 2. It excites the nervous system. 3. It promotes the coagulability of the blood, probably by increasing its fibrin. In the blood, ozone loses its peculiar properties, possibly entering into combination with some constituents of the circulating fluid. 4. Animals may be subjected to the influence of a considerable proportion of ozone in the air for hours without permanent injury, but in the end ozone produces effects which may continue after its withdrawal and destroy life.

MM. Labbé and Oudin,[81] in a communication to the Académie des Sciences, Paris, reporting their experiments in the treatment of consumptives with inhalations of ozone (ozonized air), observed, in subjects whose blood contained oxyhæmoglobin below the normal quantity, that the amount of oxyhæmoglobin was augmented one per cent at the end of a quarter of an hour's inhalation of the gas. This augmentation persists only twelve to twenty-four hours if the inhalations be discontinued. If they are renewed every day the quantity of oxyhæmoglobin continues gradually increasing until it attains the physiological figure. Inhalations in subjects with oxyhæmoglobin in normal quantity in the blood only show a slight increase.

MM. Gautier and Larat are of opinion that the production of ozone from the air by means of the electric spark is accompanied by the formation of nitrogen compounds. They doubt that ozone causes an augmentation of the oxyhæmoglobin of the blood, and quote M. D'Arsonval as authority that the coloration of the corpuscles of the blood is due to nitrogen compounds. Their clinical experiments with ozone in tuberculous and anæmic subjects were negative.

In the discussion which followed this communication

MM. Labbé and Oudin stated that their opinion as to the therapeutic value of ozone by inhalations of this gas was deduced from their own observations at the following hospitals: La Charité, Aux Enfans, La Maternité, and Des Enfans tuberculeux d'Ormesson, and from foreign sources.

M. D'Arsonval said that if the ozone produced by MM. Labbé and Oudin did not contain nitrogen compounds, then it was possible that feeble doses of ozone might have therapeutical value.[135]

Dr. Gautier,[82] in a communication to the Société pour l'Avancement des Sciences, Marseilles, September 21st, 1891, insists on the static bath and effluvia of ozone in the treatment of chronic diseases. In his former publications he lays stress upon the advantages of the static bath, which by its expansive force augments nutrition and hastens all the functions, and which, in combination with the effluvia of ozone, is able to transform the temperament of a tuberculous subject. He says: "Our aim is the modification of the organs, because the behavior of bacilli, nosologically considered, is as a product and not as a cause."

Dr. Boutens [64] (meeting of the Société pour l'Avancement des Sciences, September 23d, 1891) claims that ozone augments the quantity of oxyhæmoglobin in the blood.

Schultz[83] experimented on animals with ozone in poisonous quantity. His conclusions, drawn from the appearances of the internal organs after the death of the animals, are that, as the mucous membrane of the larynx and trachea did not show the slightest trace of any corrosive action of the ozone, the inflamed and changed condition observed of the lungs arose from impairment of the breathing caused by the recurrent cerebral irritation, which was induced by the ozone taken up by the blood and carried into circulation.

Mulvany [84] observed that ozone had a decidedly invigorating effect on the organs of procreation. There is evidence to show that its action has been to restore virility.

Binz [85] found that even in a high degree of ozone narcosis the heart and blood vessels were not perceptibly affected. This investigator also observed that ozone passed without decomposition through a solution of albumin contained in a vessel 1.85 centimetres deep. This experiment establishes the fact that ozone can not only exist in the fluids of the tissues, the greater part of which contain albumin, but is free to act in the extreme parts of the body.

Dr. C. H. Brown [86] also experimented with the action of ozone on albumin. He found that a solution of this substance was not coagulated by ozone, even when heated to the boiling point. Peroxide of hydrogen, on the contrary, coagulated albumin with considerable evolution of gas.

Engler [131] says that many competent authorities do not agree as to the physiological action of ozone, its importance in its relation to the whole process of respiration, the constitution of the blood, and the general welfare of mankind. Without committing himself to any opinion, he thinks it quite possible that atmospheric ozone may find its way into the lungs. If it be considered with what ease, for instance, chlorine, a gas likewise, and which acts with such energy on animal substances, finds its way into the lungs; and if it be remembered how difficult it is, by means of the most effectual methods of absorption, to eliminate the last trace of a gas from a gaseous mixture, it must be admitted that the air we breathe may reach the lungs in an ozonized state. The fact, observed by all who have experimented with ozone, that its odor will cling to the hands and clothing without decomposition for some length of time, is a strong argument in favor of the assumption that ozone does find its way into the lungs and blood.

Nutrition.—Dr. P. le Stunf [87] asserts that ozone acts on the blood and the nutrition.

Dr. H. S. Norris,[88] in his experiments at the City Hospital, New York, found that the patients under ozone

treatment improved in weight, showing better assimilation and consequently increased nutrition.

Dr. Arthur Ransome,[89] in his experiments with ozone inhalations, found that the patients gained in weight, showing improvement in the function of assimilation and consequent better nutrition.

Enrico di Renzi,[90] Naples, observed that inhalations of ozone were followed by increase of nutrition.

Machines for the Generation of Ozone.—The various processes by which ozone can be produced have engaged the attention of many inventive minds, with the view of constructing apparatus by which this gas could be generated in such quantity as to render it available not only for industrial purposes but also for medical use.

The aim of the apparatus invented has covered a large field, and includes machines intended for the disinfection of hospitals, rooms, and buildings; the preservation of meat, etc.; improving the quality of wines and spirits and ageing them; bleaching, etc.

Some of the apparatus devised include contrivances for the production of ozone by means of the slow oxidation of phosphorus. Among the names of inventors of such machines may be mentioned Schönbein,[91] A. W. Sangster,[92] R. S. David,[93] Radulowitsch,[94] Lake,[95] F. W. Bartlett,[96] and Leeds.[97] These contrivances do not give ozone, but rather acids exhibiting the usual iodide of potassium reactions.

Others, P. Rumine,[98] Turner and Vanderpool,[99] and Oskar Loew,[100] again have designed apparatus for the production of ozone, utilizing the application of a current of cold air passed over a Bunsen burner or a modification thereof.

There are not wanting machines making use of static electricity for the generation of ozone. Among the inventors of this class of machine may be mentioned H. D. Hall, patent July 31st, 1883.

Other inventors have attempted the production of ozone apparatus in which the gas is generated by chemicals.

The great defect in these inventions was the limited quantity of ozone yielded and the consequent expense of working them.

Carius [129] describes an apparatus for the production of ozone by the electrolysis of water with twelve to sixteen Bunsen elements. But this method yields only a small quantity of ozone, and can be recommended for use only when it is desirable to obtain ozone in concentrated condition without regard to its purity.

Apparatus by which ozone is generated by the silent discharge or electric effluvium seemed to offer the best solution of the production of ozonized oxygen or air on a large scale.

The production of a suitable effluvium has led to the construction of many arrangements, all embodying a similar principle, which consists in allowing a stream of oxygen or air to pass between dielectrics (thin plates of glass or other material of different specific inductive powers) interposed between two conductors of a certain length placed parallel. The electricity of high tension is discharged in an effluvium through the glass plates and the oxygen or air passing between them, which is then ozonized.

The pioneer attempt to produce ozone by electricity was the apparatus devised by Schönbein,[101] and afterward used by Marignac, De la Rive, Frémy and Becquerel, Andrews, and others. It consisted of a tube arranged somewhat after the manner of a eudiometer, filled with air or, better still, oxygen, placed over mercury or sulphuric acid. Two platinum wires, melted into the tube, were placed in communication with the poles of an induction coil. The electric spark passing between them effected the ozonization of the oxygen. By this method only a proportionately small amount of ozone is obtainable.

The first apparatus utilizing the generation of ozone by the electric effluvium was that of Von Babo,[102] modified by Houzeau. This apparatus consists of a glass tube

of from one to two millimetres in thickness and forty centimetres long. A platinum wire of somewhat large diameter is placed along the axis of the tube, and a second platinum wire of small diameter is wound spirally round the outside of the same tube. The two wires are placed in communication with the two poles of a Ruhmkorff coil. In order to obtain ozonized oxygen a slow current of this gas is passed into the tube.

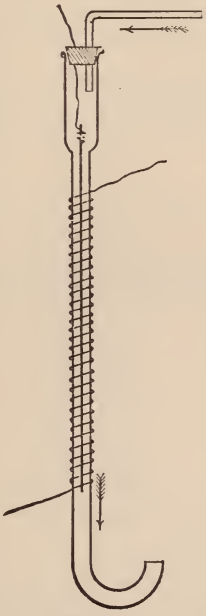

FIG. 1.

Werner Siemens[109] also devised an apparatus consisting of two concentric glass tubes melted together near the top and provided with two smaller tubes, one at each end, for the passage of the gas through the annular space formed by the concentric tubes. The internal tube is covered with tinfoil on the inner side, the external tube being similarly treated on the outside. The whole is placed in a vessel of water, water also being contained in the inner tube, one electrode dipping into each re-

spectively. It will be apparent that in this apparatus the effluvium must traverse the two surfaces of glass and the volume of gas passing through.

Ruhmkorff [104] constructed an apparatus formed of parallel plates of glass separated by a very small interval, each plate having a sheet of tin at its upper part. These were joined up in pairs with two metallic buttons connected with the poles of a bobbin. A large number of these plates, superimposed and enclosed in a box furnished with tubulures, will produce the effluvium over a very wide surface.

Siemens' arrangement seems to have suggested the arrangement of D'Arnoult, Thenard, and Bertholot.

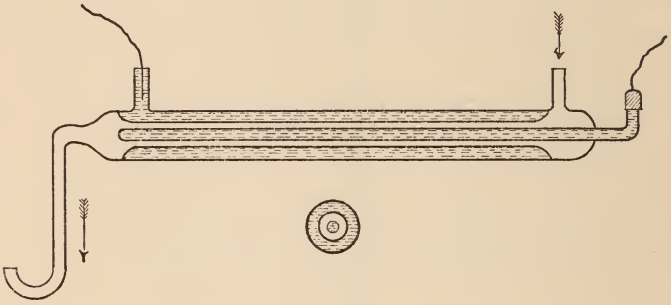

FIG. 2.

Of these, Thenard's [105] tube is made up of three concentric tubes, properly joined so as to present three spaces, one cylindrical and the other two annular, separated by glass partitions. The central tube contains conducting liquid, serving as an armature. The outer annular space is filled with the same liquid, which consists of chloride of antimony dissolved in hydrochloric or concentrated sulphuric acid. Two platinum wires, in connection with the poles of an induction coil, serve to convey the electric current to the conducting liquids. The oxygen or air to be ozonized passes through the intermediate annular space. The apparatus in use is placed horizontally.

The Bertholot [106] apparatus consists of a thin glass

tube thirty millimetres in diameter and from thirty to thirty-five centimetres in length, closed at the lower end. It is provided with two eduction tubes, one at each extremity. Another thin glass tube, also with closed lower end, is placed in the axis of the larger tube, a little above the upper eduction tube, and fixed to it, the connection of the two tubes being ground with emery to insure perfect junction. The inner tube is filled with a conducting liquid, into which one of the electrodes is introduced. The whole arrangement is put into a glass vessel also containing a conducting liquid, the other electrode dipping into it. The apparatus must be well centred, so that the two surfaces may be equidistant. The oxygen or air passes by way of the eduction tubes through the annular space formed by the two concentric tubes.

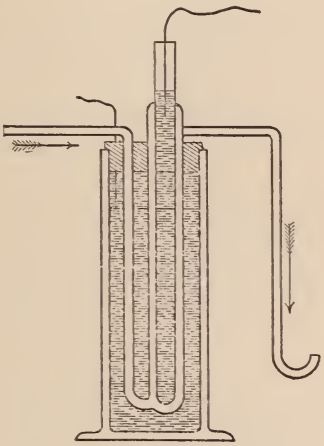

FIG. 3.

Tisley, of London,[107] constructed an ozone generator, devised by Wills, a modification of Siemens', and which permits of refrigerating the oxygen passing under the influence of the electric discharge. This was effected by means of a current of cold water. It was claimed that this arrangement prevented the partial decomposition of

the ozone caused by the heating of the tube by the electric current.

Brin Brothers,[108] of London and Paris, are the inventors of an apparatus generating ozone. They employ the Bertholot tubes arranged in series.

Among other inventors of ozone machines and apparatus may be mentioned Boillot,[109] Wislicenus,[110] Kolbe,[111] Yost,[112] Fahrig,[113] Schereller and Weise,[114] and Andreoli.

The rapid strides of electric science, and the facility with which currents of high tension are now obtained,

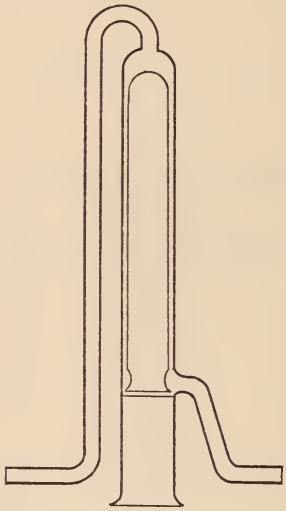

FIG. 4.—Ozone tube for water electrodes.

have also resulted in the development of more perfect apparatus for the production of ozone, and the Ozone Company of this city have established a plant for its manufacture on a commercial scale.

They are the makers of an improved ozone machine which is specially adapted to the needs of the medical profession and may be used for the administration of ozone by inhalation. The machine is particularly suitable for the diffusion of ozonized air in the wards of hospitals, either for disinfecting purposes or for the treatment of consumptive patients.

The Ozone Company's machine consists of motor, blower for forcing the air to be ozonized through the apparatus, transformer, and the ozone generator proper. The motor, which may be driven by the current available from any electric plant, supplies also a derived current to the generator by means of a commutator of special construction. High potential is secured by passing the current through a specially wound transformer before it reaches the generator. The motor operates a small blower, which forces the air through an ingeniously devised drying tube and thence to the ozone-generating tubes. The generator is formed of a group of ozone tubes arranged in a circle. A modification of the Siemens tube is employed. The amount of ozone given off by this machine is very great, and will in a very short time make itself apparent, by its characteristic odor, in the most distant parts of a large room or hall. The whole machine occupies only a small space.

Further Comments upon the Value of Ozone in Therapeutics.—The therapeutic value of ozone has been a subject of dispute ever since its discovery by Schönbein, and much has been written *pro* and *con* by advocates and opponents.

Prominent among diseases which it has been considered would be benefited by ozone is tuberculosis.

During the past two or three years many practitioners have been experimenting with ozone in the treatment of this disease. Judging from the data now at hand the results have been satisfactory and worthy of continued effort.

The labors of MM. Labbé and Oudin [116] have already been referred to. They establish the fact that the administration of chemically pure ozone (ozonized air) inhalations has curative value. Their statements are verified by the report of Dr. Boutens,[115] of St. Raphael, who, as a result of his experiments, concludes that ozonized air (one-tenth of a milligramme to the litre of air) is, when inhaled, absolutely innocuous, that it possesses a

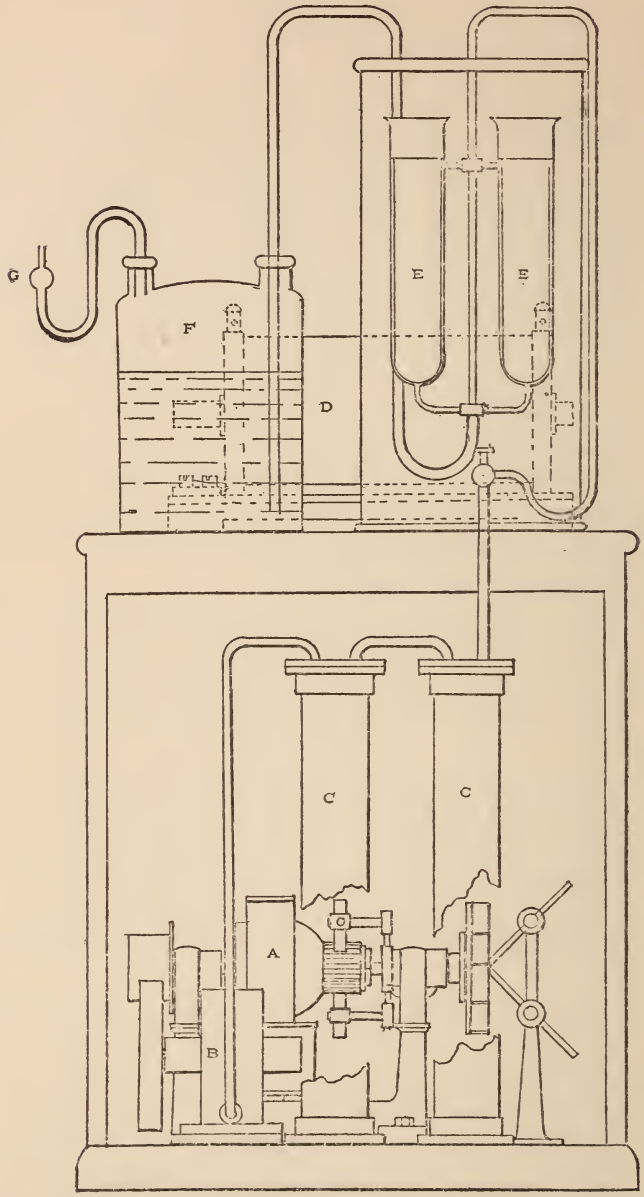

FIG. 5.—Ozone machine. A, motor; B, blower; C, C, drying tubes; D, transformer; E, E, ozone generators; F, wash bottle; G, goose-neck inhaling tube.

constant curative action in tuberculosis, and augments the quantity of the oxyhæmoglobin in the blood.

Dr. Augustus Caillé, of this city, has experimented with the small ozone generators of Labbé and Oudin.[116] He reports that in three typical cases of limited apex tuberculosis a local improvement was not obtained; that in pertussis the inhalations have a very distinct curative effect, while in chlorosis and anæmia inhalations are exceedingly valuable and give better and prompter results than any other form of medication; and, finally, that daily inhalations increase the quantity of oxyhæmoglobin in the blood from one to four per cent in a short time.

Dr. Caillé's observed fourteen reported cases are worthy of careful study.

Dr. Arthur Ransome,[117] of Liverpool, treated consumptive patients with inhalations of ozone. He published the results of his treatment, which may be summed up as follows:

Thirteen cases treated—two in the third stage; most of them under observation for more than a year; two of them for two years; only two have distinctly deteriorated in the time, and one of them has died of pharyngeal phthisis.

He found during treatment that there was continued freedom from fever, absence of night sweats, diminution in the amount of expectoration, improvement of appetite and in sleeping power, and much consequent gain in weight.

He also states that, if his repeated microscopic examinations were to be trusted, the ozone diminished the number of bacilli in the expectoration.

According to Dr. P. Le Stunf,[119] ozone generated by electricity, and in the proportion of one-tenth of a milligramme to the litre of air, is quite innocuous, not only to animals, but to human beings in health and disease. By inhalation it increases the oxyhæmoglobin from one to two per cent. Its action is well borne by tuberculous subjects when administered in therapeutic doses. After

fifteen to twenty sittings improvement is perceptible. It is a question with him as to whether it does not act as a microbicide in consumptive individuals. It acts on the blood and nutrition. The appetite returns, the digestion improves, the night sweats diminish; sleeping power sometimes reappears, even in patients that have long been tormented by insomnia. There was also an amendment of pulmonary lesions, real retrogression of the local phenomena.

Dr. Otto Ringk's [120] experience is that ozone (ozone water) treatment in tuberculosis gave excellent results.

Dr. Henry S. Norris, of this city, treated thirteen cases of phthisis with ozone water. Of this number, five who had extensive disease were not improved; in seven marked improvement took place—increase in weight, diminution and cessation of cough and expectoration, termination of night sweats.

Dr. C. E. Quimby, Dr. W. B. Wood, Dr. Conkling, as well as several other gentlemen of this city, have also, I am informed, been investigating the therapeutic properties of ozone, and will, no doubt, soon report their experiences.

Aquozon and kodozonol (ozonized cod-liver oil) have been used at the French Hospital, New York. It is stated that the patient showed a steady gain in weight, owing to increased assimilation.

Diabetes.—Dr. Emil Schnee, of Carlsbad, is of the opinion that ozone is the remedy *par excellence* for diabetes. He states that diabetes is based on the incapacity of the protoplasm for the production of carbonic acid. The use of ozone places the physician in a position to supply this lack of carbonic acid. He has prescribed aquozon to his patients both in this country and in Europe with happy results.

Dr. C. H. Avery, Secretary of the New York County Medical Society, used aquozon on himself in the treatment of this disease. He reports as follows: "I am carefully watching the action of the ozone water in my own case.

I first discovered sugar in May, 1885, and have resorted to many remedies. Sugar has been constant but in varying quantity, although starchy foods have been strictly avoided. My observations thus far as to the effect of the water are:

"1. A decided diminution of thirst, so that I used no more than a pint of water for twenty-four hours.

"2. An increase of vital force and muscular strength; a general impression of feeling better.

"3. It procures sound and refreshing sleep, and allays nervous irritability. This is specially noticeable.

"During the past week there seems to be a slightly less amount of sugar, amounting to about 3° specific gravity. It is not to be expected that in a case of so long standing sugar will disappear so soon."

Private reports to me from physicians who have experimented with aquozon in diabetes show that marked improvement has taken place.

Cholera.—As cited above, many observers have insisted that there is a direct relation between the appearance of an epidemic of cholera and a diminution in the amount of ozone in the atmosphere. In 1883 Dr. Onimus,[121] during the cholera epidemic, made some experiments at Marseilles in order to personally satisfy himself as to the correctness of the assertions of previous investigators, and also to establish, if possible, the therapeutic value of ozone in the treatment of this infection. These experiments were conducted at the instigation of M. Gauckler, manager of La Compagnie des Chemins de Fer de l'État. This gentleman, while on the central committee of meteorology in Alsace in 1865, was much struck by the coincidences in the infected localities. He placed at the disposal of Dr. Onimus the material and staff of La Compagnie P. L. M., in order to carry out the necessary investigations. An ozone plant was installed at the cholera hospital, and the gas was allowed to diffuse itself slowly in one ward, while another, for comparison, was kept free of ozone. Dr. Onimus found that in the male ward no deaths took place

during the two days that it was kept under the influence of ozone. He then experimented in the female ward, with the result that during five days only two deaths occurred, and one of these was a woman who gave birth to a dead child. In his pamphlet on the subject he says: "We can certify that this method—I hesitate to say medication—gives excellent results."

BIBLIOGRAPHY.

1. POGGEND. Annalen, xci., 625.

2. Beschreibung einer Elektrisirmaschine, von MARTINUS VAN MARUM, deutsch. Uebersetzung, Leipzig b. Schwickert, 1786, I. Theil. 25 und 28, I. Fortsetzung, 1788, 39; SAXTORPH'S Elektricitätslehre, übersetzt von FANGL., I. Theil, 434.

3. GILBERT'S Annalen, vii., S. 107.

4. Berichte über die Verhandlungen der naturforsch. Gesell. in Basel, iv. 58 und 66; Abhandlungen der II. Classe der bayerischen Akademie der Wissenschaft, Bd. iii., Abth. I., S. 257; POGGEND. Annalen, l., 616.

5. Berichte über die Verhandlungen der naturforsch. Gesell. in Basel, vi. 16; Ueber die Erzeugung des Ozons auf chemischen Wege, von CHR. FR. SCHÖNBEIN, Basel, 1844, S. 155.

6. POGGEND. Annalen, liv., 402; POGGEND. Annalen, lxv., S. 75; Comptes rendus, xx., 1291; Comptes rendus, xx., 808; Aus Archiv ph. nat., i., 81, im Jahresbericht für Chemie, 1857, 78; Aus N. Archiv ph. nat., xii., 155, im Jahresbericht für Chemie, 1861, 101; Aus Archiv ph. nat., xix., 294, im Jahresbericht für Chemie, 1852, 303; Traité d'électricité, ii., 417.

7. Annal. Chim. phys. (3), xxxv., 62; Annal. Chim. Pharm., lxxxiv, 206; Comptes rendus, xxxiv., 399.

8. Lond. Soc. Proceed., ix., 606; Phil. Mag. (4), xvii., 435; Annalen de Chemie und Pharm., cxii., 185; Phil. Mag. (4), x., 455; Annalen der Chemie und Pharm., xcvii., 371; Jour. prakt. Chemie, lxvii., 494; POGGEND. Annalen, xcviii., 435; London Royal Proceed., viii., 498; Phil. Mag. (4), xv., 146; Annalen der Chemie und Pharm., civ., 128; POGGEND. Annalen, cii., 625.

9. Annalen der Chemie und Pharm., Suppl. ii., 265; Bull. Soc. chim., vi., 340.

10. POGGEND. Annalen, lxv., S. 190; lxvi., S. 163, 168, 593.

11. POGGEND. Annalen, lxxxix., 38; Annal. Chem. Pharm., lxxxviii., 221; Journ. prakt. Chemie, lix., 350; POGGEND. Annalen, xcix., 88; Annalen Chemie und Pharm., ci., 88; Journ. prakt. Chemie, lxx., 446.

12. Annalen der Chemie und Pharm., cxxvii., 38; Journ. für prakt. Chemie, xc., 216; POGGEND. Annalen, cxviii., 623; Comptes rendus, lvi. 390; Comptes rendus, lvii., 604; Annalen der Chemie und Pharm., cxxx., 95; POGGEND. Annalen, cxxi., 268; Phil. Mag. (4), xxvi., 554; Annal. de Chim. phys. (4), vii., 113; Comptes rendus, lxi., 941; Annal. der Chemie und

Pharm., cxxxviii., 45; Chemisches Centralblatt, 1866, 33; Phil. Mag. (4), xxxi., 82; POGGEND. Annalen, cxli., 294; Chemisches Centralblatt, 1870, 385.

13. Chemisches Centralblatt, 1881, No. 28.

14. Annal. de Chim. phys. (4), xxvii., 60.

15. Untersuchungen über den Sauerstoff, Hannover bei HAHN, 1863; Neue Untersuchungen über den elektrischen Sauerstoff, Göttingen bei DIETRICH, 1869; Jahresbericht der Chemie, 1863, 126

16. Annalen der Chemie und Pharm., clxi., 232.

17 Bericht der deutschen chemischen Gesellschaft, 1875, 905.

18. Comptes rendus. xlii., 941; xliii., 216.

19. Instit., 1856, 282; Jahresbericht für Chemie, 1856, 267.

20. Comptes rendus, lv., 731.

21. Ueber den Einfluss der Wälder auf die Beschaffenheit der Luft, EBERMEYER, 1873, und Monit. scientif. (3), iii., 895.

22. SAINTPIERRE, Comptes rendus, lviii., 420; SAINTPIERRE, Chemisches Centralblatt, 1864, 697.

23. SCHÖNBEIN, Verhandlungen der naturforsch. Gesell. in Basel, ii., 35; Journ. prakt. Chemie, liv , 65.

24. Bericht der deutschen chemischen Gesellschaft, 1875, 905; 1876, 581; Gazz. chim. ital.. 1876, 88.

25. Comptes rendus, xl., 947; Journ. für prakt. Chemie, lxv., 499; POGGEND. Annalen, xcv., 484; Annalen der Chemie und Pharm., cliv., 215; Aus CIMENTO, ii., 291, im Jahresbericht für Chemie, 1855, 287; SCHÖNBEIN, Verhandlungen der naturforsch. Gesell. in Basel, iii., 305; Journ. prakt. Chemie, lxxxvi., 70; BÖTTGER, Journ. prakt. Chemie, lxxxvi., 377, und Jahresbericht des physik. Vereins in Frankfurt, 1859-1860; Verhandlungen der naturforsch. Gesell. in Basel, i., 246; Journ. prakt. Chemie, lxvi., 280; Annalen Chem. u. Pharm., cxlii., 107; Chemical News, xxv., 242; Journ. prakt. Chemie. lxxxvi., 377; Aus Bull. Soc. chim., Sitzung 11. Mai 1860, im Jahresbericht für Chemie, 1860, 66.

26. Verhandlungen der naturforsch. Gesell. in Basel, i., 252; Journ. prakt. Chemie, lxvi., 286.

27. Chemical News, xxv., 242.

28. Bericht über die Verhandlungen der naturforsch. Gesell. in Basel, vi., 16.

29. BOETTGER, Chemisches Centralblatt, 1870, 161; BOECKE, Chemical News, xxii., 57.

30. Bericht über die Verhandlungen der naturforsch. Gesell. in Basel, iv., 58, 66; Abhandlungen der II. Classe der bayer. Akad. der Wiss., III. Abtheilung, i., 257; POGGEND. Annalen, l., 616.

31. Bericht der deutschen chemischen Gesell., 1876, 84; Chemical News, xxvi., 113; Phil. Mag. (4), xliv., 156.

32 POGGEND. Annalen, cii., 120; Nature, viii., 148; Annalen der Chemie und Pharm., ii. Suppl., 267; ENGLER UND NASSE, Annalen der Chemie und Pharm., cliv., 215; Comptes rendus, lxxiv., 256; Annal. de Chim. phys. (4), xxii., 150; Dictionn. de Chim., WURTZ, ii*a*, 719; Comptes rendus, lxxv., 214 und 1712; Comptes rendus, lxxv., 118; Bericht einer akad. Commission

über das Apparat., ibid., 1735; REGNAULT-STRECKER'S Lehrbuch der anorg. Chemie, IX. Auflage von WISLICENUS, S. 73; Kurzes Lehrbuch der anorg. Chemie, von H. KOLBE, S. 106; Annal. de Chim. phys. (5), x., 165; Chemical News, xxvi., 113; Phil. Mag. (4), xliv., 156; Bericht der deutschen chemischen Gesell., 1876, 84; Gazz. chim. ital., 1876, 88.

33. DR. O. RINGK, Wie können wir Ansteckungskrankheiten als Diphtheritis, etc., mit Erfolg entgegentreten? 1890, S. 71.

34. Annal. de Chim. phys. (4), vii., 113; Comptes rendus, lxi., 941; Annal. der Chemie und Pharm., cxxxviii., 45; Chemisches Centralblatt, 1866, 33; Phil. Mag. (4), xxxi., 82.

35. DR. O. RINGK, as above cited.

36. Comptes rendus, xci., Sur la liquéfaction de l'ozone.

37. Pharmaceutical Era, 1890, On the Liquefaction of Ozone.

38. L'Ozone, ou recherches chimiques, etc., 1856, p. 37.

39. SCHÖNBEIN, Poggend. Annalen, lxvi., 293; MARIGNAC, Annal. de Chim. phys. (3), xiv., 254; ANDREWS, Poggend. Annalen, xcviii., 435; C. HOFFMANN, ibid., cxxxii., 617; BOETTGER, Chemisches Centralblatt, 1872, 112, und 1874, 386; FOX, Ozone and Antozone, London, 1873, S. 29; RAMMELSBERG, Bericht der deutschen chemischen Gesell., 1873, 603; Annalen der Chemie und Pharm., liv., 130.

40. Annalen der Chemie und Pharm., cliv., 215.

41. Annal. de Chim. phys. (4), xxvii., 15.

42. Bericht der deutschen chemischen Gesell., 1872, 520, und 1873, 806; Annalen der Chemie und Pharm., clxxiv., 1

43. Bericht der deutschen chemischen Gesell., 1873, 1224.

44. Bericht der deutschen chemischen Gesell., 1879, 1831.

45. SCOUTETTEN, L'Ozone, ou recherches chimiques, etc., 1856.

46. DR. O. RINGK, above cited (see 33).

47. ENGLER, Historisch-kritische Studien über das Ozon, 1879, S. 38.

48. Scientific American Suppl., Dec. 12th, 1891 (quoted from La Nature).

49. ENGLER, V. GORUP-BESANEZ, quoted, above-cited treatise (47), p. 39.

50. SCOUTETTEN, above cited (45), p. 38.

51. DR. RINGK, above cited (33), p. 73.

52. Comptes rendus, lxxv., 349.

53. ENGLER, above cited (47), p. 37.

54. Zeitschrift für nat. Medicin, Bd. xv., 391.

55. Ozone and Antozone, by FOX, 30.

56. SCOUTETTEN, above cited, p. 182.

57. SCOUTETTEN, above cited, p. 188.

58. SCHMIDT'S Jahrbücher, 1875, 1; Sitzungs-Bericht der niederrheinischen Gesell. für Natur und Heilkunde, Bonn, January, 1875; Botanischer Jahresbericht, v., 88.

59. Bull. de la Soc. chim. de Paris, 1881, t. xxxv.

60. BOILLOT, Comptes rendus, t. lxxxi., 1875.

61. D. A. VAN BASTELAER, L'Ozone atmosphérique; Acad. Royal de Belgique, séance 24 Septembre 1892, cites SCHÖNBEIN, CLEMENT (1849), RICHARDSON (1863).

62. Ozone, Scientific American Supplement, January 23d, 1892 (from La Nature).

63. Ueber die Einwirkung des Ozons auf Bakterien, Arbeiten des königl. Gesundheitsamte, Bd. viii,, 229.

64. De l'ozone en thérapeutique, séance 23 Septembre, 1891, Assoc. par l'Avancement des Sciences, congrès à Marseille, Progrès médical, t. xiv., p. 232.

65. Ueber die Einwirkung des Ozons auf Bakterien, Jahresbericht, VIRCHOW, 1889.

66. Ueber den Einfluss Ozons auf das Wachsthum der Bakterien.

67. Ozone and Antozone, by FOX, 127; On Meteorology in relation to Epidemic and Sporadic Cholera, by MOFFATT, Lancet, September 9th, 1865.

68. British Association Report, 1869; FOX, p. 130.

69. Ozone and Antozone, FOX, pp. 130–136.

70. Ueber die Cholera, 1865, FOX. p. 135.

71. Ozone et Choléra, par le DR. ONIMUS, 1884, G. MASSON, Paris.

72. Ibid., p. 18; Verhandlungen der naturforsch. Gesell. in Basel, i., 457, und ii., 9; see SCHÖNBEIN, Verhandlungen der naturforsch. Gesell. in Basel, ii., 14.

73. DR. O. RINGK, above cited (33), p. 58.

74. Phys. Chem., vol. i., 1880.

75. Acad. de Médecine, Paris, séance Aout 11, 1891.

76. Société de Biologie, séance du 16 Mai, 1891, Progrès médical, t. xiii., 1891, p. 430.

77. Quoted by DR. O. RINGK in his treatise, see (33), p. 24.

78. Virchow's Archiv, 1884, i., 135.

79. Cited above (33), p. 17.

80. Quoted by DR. O. RINGK, see (33), p. 20.

81. Jahresbericht. Ueber die Leistungen und Fortschritte in der gesammt. Medizin, Virchow's Archiv, 1891; Comptes rendus; Progrès médical, t. xiv., 86, 1891; Séance de l'Acad. de Sciences, 20 Juillet, 1891.

82. Assoc. par l'Avancement des Sciences, congrès à Marseille, 21 Septembre, 1891; Progrès médical, t. xiv., p. 230.

83. Archiv für exp. Path. und Pharm., 1892, xxix., p. 344; Sur l'empoisonnement chronique par l'ozone, Revue Internationale de Bibliographie, No 17, 1892; Therapeutische Mittheilungen aus Vereinen, O. LIEBREICH, March, 1892.

84. Quoted by DR. SAMUEL S. WALLIAN, A Study in Aerotherapeutics, New York Medical Journal, July 25th, 1891, vol. liv., No. 660, p. 101.

85. Berliner klinische Wochenschrift, 1882, Ozonized Air as a Narcotic.

86. The Chemical Relation of Ozone and Hydrogen Peroxide, by DR. CHARLES H. BROWN, New York.

87. Merck's Bulletin, September, 1892.

88. New York Medical Journal, November 5th, 1892, The Internal Administration of Ozone in the Treatment of Phthisis (report of cases).

89. Medical Chronicle, May, 1889, No. 56, p. 797; also April, 1888.

90. Archiv für pathologischen Anatomie und Physiologie, Bd. civ., Ueber den Ozon.

91. ENGLER, above cited (47), p. 34.

92. Bericht der deutschen chemischen Gesell., 1878, 815; United States Patent 199,997, October 29th, 1877.

93. Bericht der deutschen chemischen Gesell., 1875, 275; WAGNER'S Jahresbericht, 1875, 463; English Patent No. 3,190, October 2d, 1873.

94. Bericht der deutschen chemischen Gesell., 1877, 2059.

95. Ibid., 1877, 904; English Patent No. 3,294, September 20th, 1875.

96. Ibid., 1878, 1469; English Patent No. 2,697, June 12th, 1877.

97. Annalen der Chemie und Pharm., cxcviii., 30.

98. Bericht der deutschen chemischen Gesell., 1872, 123.

99. Ibid., 1873, 1553.

100. Dingl. polyt. Journ., ccxiii., 130; Jahresbericht von WAGNER, 1874, 404; Bericht der deutschen chemischen Gesell., 1872, 740; French Patent 92,134, July 4th, 1871.

101. ENGLER, cited above (47), p. 31.

102. Annalen der Chemie und Pharm., II. Supplement, 267.

103. POGGEND. Annalen, cii., 120.

104. The Formation of Ozone by Electric Discharges, Scientific American Supplement, April 26th, 1890 (from La Lumière Électrique).

105. Comptes rendus, lxxv., 118; Bericht einer akad. Commission über den Apparat, ibid., 1735; Scientific American Supplement, above quoted (104).

106. Annales de Chim. phys. (5), x., 165; Scientific American Supplement, cited above (104).

107. Bericht der deutschen chemischen Gesell., 1873, 769; Nature, viii., 148.

108. Scientific American; English Patent No. 11,846, September 17th, 1886.

109. Comptes rendus, lxxv., 214 and 1712.

110. REGNAULT-STRECKER'S Lehrbuch der anorg. Chemie, IX. Auflage, WISLICENUS, S. 73.

111. Kurzes Lehrbuch der anorg. Chemie, von H. KOLBE, S. 106.

112. United States Patent No. 254,424. February 28th, 1882.

113. Scientific American Supplement No. 873, September 24th, 1892.

114. Electric Review, New York, October, 1892.

115. Assoc. par l'Avancement des Sciences, congrès à Marseille, September 23d, 1891; Progrès médical, t. xiv., 1891, p. 232.

116. See 81.

117. See 89.

118. See 82.

119. See 87.

120. See 33.

121. See 71.

122. Scientific American, No. 222, April 3d, 1880; Lines of Discovery in the History of Ozone, by ALBERT R. LEEDS, Ph.D.; ODLING, Monit. Scientif. (3), iii., 324.

123. See 104.

124. Bericht über die Verhandlungen der naturforsch. Gesell. in Basel, iv.,

58, 66; Abhandlungen der II. Classe der bayerischen Akademie der Wissenschaft, III. Abtheilung, i., 257; POGGEND. Annalen, l., 616.

125. Untersuchungen über den Sauerstoff, MEISSNER, Hannover bei Hahn, 1863, S. 122.

126. Annalen der Chemie und Pharm., cliv., 215.

127. Annalen der Chemie und Pharm., clxxiv., 1.

128. Bericht der deutschen Gesell,, 1873, 1225.

129. Annalen der Chemie und Pharm., clxxiv., 6.

130. ENGLER UND MASSE, Annalen der Chemie und Pharm., cliv., 215; SCHOENE, ibid., cxcvi., 240.

131. ENGLER, above cited (47), p. 58.

132. DR. O. RINGK, above cited (33), p. 15.

133. New Yorker Staats-Zeitung, April 27th, 1890, WILHELM STOSS.

134. Revue internationale d'Electrothèrapie, July, 1892; M. HERMAN, Du pouvoir bactéricide de l'ozone, Annales de la Soc. medico-chir. de Liège.

135. Revue internationale d'Electrothérapie, February, 1892; Progrès médical, March 5th, 1892.

136. Experiments as to the Value of Nascent Ozone in certain forms of Diseases of Children, with Demonstration of an efficient Generator, AUGUSTUS CAILLÉ, M.D., Archives of Pædiatrics, August, 1892.

137. Cholera: its Prevention and Treatment, ELMER LEE, A.M., M.D., Chicago Clinical Review, April, 1893; Hydrogen Peroxide in Contagious Diseases, CYRUS EDSON, M.D., The Doctor of Hygiene, April, 1893.

138. Encyclopædia Britannica, 9th ed., vol. v., p. 417.

139. Lecture on Liquid Air, Royal Institution, February 22d, 1893.

140. KINGZETT, 1888.

141. BAUMANN, Zeitschrift für physiologische Chemie, von GALEZOWSKI, Rundschau, 1889.

142. American Therapist, January 15th, 1893.

143. Chemical News, March 7th, 1863.

DISCUSSION.

DR. A. CAILLÉ said, with regard to the cases referred to in the paper as treated with ozone by himself, that he used a French apparatus of a size convenient for home use. It generated ozone from the air by electricity.

It was about two years ago that he picked out three typical cases of apex tuberculosis and subjected the patients to ozone inhalation a long time, with the result of improvement in the general condition in all the cases. The oxyhæmoglobin increased three or four per cent, the appetite improved, night sweats disappeared, but up to the time of the report there was no noticeable improvement in the local signs. That was a year ago, and since then he had kept one patient under treatment by leaving

a machine at her home and giving instructions to inhale the ozone three times a day in a nascent state. This she had done, no other treatment had been employed, and he was now able to report improvement in the local symptoms. The case had shown tuberculosis of the entire left upper lobe, there had been abundant expectoration with tubercular bacilli; but the cough had disappeared, there was no expectoration, the moist râles had disappeared, and there remained only the physical signs of the so-called fibrinous or dry process. When the patient began treatment she weighed one hundred and eighteen pounds; at present she weighed one hundred and forty. There had been absolutely no cough during the last three months.

In simple anæmia and chlorosis there was no doubt but what ozone was beneficial. The oxyhæmoglobin increased within a short time, the pallor of the mucous membranes disappeared, the appetite increased, the buoyancy of spirits was enhanced. He had also found ozone decidedly valuable in several cases of pertussis.

He was somewhat sceptical as to the value of some of the preparations in the market. Ozone certainly tended to decompose when brought in contact with organic matter, and he could not understand how it could retain its oxidizing properties when kept in oil or other organic menstrua.

DR. W. H. PARK referred to the experiments with the germicidal properties of ozone which he had carried out at the request of Dr. Morton, and said that not only was the destruction of the bacteria contained in the water through which the ozone was passed very rapid, but also the changes in the appearance of the fluid, the turbid water being in a few minutes rendered quite clear.

The effect on the bacteria of placing them in a current of ozonized air was somewhat disappointing, as it took an exposure of at least forty-five minutes to kill even a portion of the bacteria.

DR. W. H. DRAPER expressed his obligation to Dr. Morton for having brought this interesting subject before the Academy. He had had no experience with ozone, but had long used peroxide of hydrogen as a disinfectant, and he believed that as a destroyer of germs, especially in affections of the mucous membrane of the throat and mouth, it was of great value. It had occurred to him also that ozone might be useful by inhalation in

phthisis, as had been suggested by Dr. Morton. He believed it was generally recognized now that phthisis was a mixed infection, at least in the second and third stages, not only of the tubercle bacillus, but also of the staphylococcus and streptococcus. The demonstrations of Dr. Morton and of others had shown ozone to be very destructive of the staphylococcus and streptococcus, and it seemed probable that for this reason it might be a valuable agent in phthisis. How far ozonized air or ozone pure was capable of destroying the tubercle bacillus he did not know, but in phthisis, after it had reached the stage of mixed infection, he believed it would prove a useful remedy. He also believed that it would be a valuable inhalation in gangrenous affections of the lungs, as a deodorizer of the breath if not as an antiseptic.

DR. W. J. MORTON, in closing the discussion, said, in reply to Dr. Caillé, that certain of the ordinary oils would take up ozone as ozone, without being acted upon chemically by this agent; that is to say, the oil seemed to absorb the ozone without a chemical combination taking place, as was shown by tests for ozone made at variable periods subsequently. This absorbing power certainly was true of aquozone, although this preparation carried only a small volume of ozone. Tests made by Witthaus had shown conclusively that water containing a small percentage of phosphate of soda held ozone a number of months. The compounds of ozone with organic matters were scarcely understood at all.

He had been glad to have the confirmation of Dr. Park of the experiments which they had made as to the germicidal power of ozone. It had killed the staphylococcus pyogenes aureus in five minutes, one proof of which was the immediate clearing up of the water.

Replying to a question, he said the usual strength of the inhalation had been about one-tenth of a milligramme per litre. By modern methods the ozonized air could be measured very carefully, so that it could be known exactly what the patient was inhaling.

A PLAN TO PROVIDE BEDSIDE INSTRUCTION IN CONTAGIOUS DISEASES IN NEW YORK CITY.

BY J. W. BRANNAN, M.D.

AT the last meeting of this Academy, held two weeks ago, a representative of the New York Board of Health told us of the workings of its Bureau of Disinfection and of its manner of dealing with cases of contagious disease in this city.

It was demonstrated to us that not only is the Board of Health prepared to handle all cases of infectious disease of which it is notified, but also that its methods of disinfection are now so efficient that there is no danger of spreading the disease by those engaged in caring for the patients.

Details were given of the arrangements at the Willard Parker Hospital for receiving and treating patients suffering from the various contagious diseases. It was also stated that two new hospitals were in process of construction, which would increase the number of beds to two hundred and fifty instead of the present sixty of the Willard Parker.

An appeal was made to the medical profession of the city to support the Health Board in its efforts to prevent the spread of contagious disease in New York.

In the course of the discussion which followed the reading of Dr. Biggs' paper it was shown that the good work of the Board of Health was fully appreciated by the profession at large. All were of one accord in approving what had been done and in promising earnest support of the measures proposed for the future. Many suggestions were made looking to greater efficiency of the present service, as well as to providing additional facilities for receiving and isolating patients.

I had the pleasure of listening to Dr. Biggs' paper, as well as to the discussion it called forth, and at the time thought that there was nothing further to be said upon the subject. Since that evening, however, it has occurred to me that the hospitals of the Health Department might serve other purposes in addition to those to which they are now devoted.

It is a fact known to us, though not appreciated by the general public, that medical students are entirely without practical instruction in contagious diseases. In all other branches of clinical medicine bedside demonstration has to a great extent replaced didactic teaching, but the student still graduates without ever having seen a case of measles, or scarlet fever, or small-pox. The reason of this is not far to seek. The general hospitals, to which students are admitted, do not receive cases of contagious disease, and the special hospitals designed for the reception of these cases do not admit students. It seems to me that this state of things may now be remedied, in view of the efficiency of our modern methods of disinfection as detailed by Dr. Biggs.

I have discussed the subject with the members of the Board of Health, and they are ready to entertain the suggestion, provided we can devise a plan that is practicable and safe.

Let us first consider the objections to utilizing the material in these hospitals for bedside instruction.

There is, of course, danger that the students may carry contagion from the wards to their homes. But this danger, I believe, can be almost entirely done away with by requiring the students to observe all the precautions and regulations under which the service is now conducted. At present the resident physicians and attendants wear special gowns and hoods and overshoes when within the wards, and carefully disinfect the hands on leaving the wards. The observance of these precautions on the part of the students will be of service to them in their future private practice.

It is also objected that the patients in these hospitals may be unwilling to serve for clinical instruction. But this objection would apply with equal force to bedside teaching in general hospitals; and we know by experience that hospital patients, as a rule, are not disturbed by the presence of students, but often welcome them as a break in the monotony of their ward life. In this connection I should recommend that the students should be admitted only in limited classes, and the privilege should be restricted to such students as have reached their third year in a graded course of instruction.

It is hardly necessary to consider in detail the advantages to be derived from opening these hospitals in the manner proposed. Training in diagnosis is the most evident gain that would result to the student. The eruptive fevers are especially adapted for demonstration, as the characteristic lesion is on the surface. And it is in just this class of disease that prompt diagnosis is most important, so that measures may be taken to protect the patient's family from infection. In non-contagious diseases a delay of two or three days in making a positive diagnosis is not usually of great moment, whereas a few hours' delay may be far-reaching in its disastrous results when we are dealing with a case of small-pox or typhus fever.

Students would also be taught how to treat the various forms of contagious disease. And as a part of this treatment they would be properly drilled in the best methods of disinfection. At present there is no uniformity in the measures taken by physicians in private practice to lessen the spread of contagion. Some weary their patients and themselves by their minute regulations, while others believe that all precautions are unnecessary or are of no avail, and hence employ none.

Who shall teach the students? This is the most difficult problem we have to deal with in this matter. There are no visiting physicians connected with the hospitals,

and the resident physicians have no time to devote to the purpose.

There are, however, a number of medical inspectors in the employ of the Health Department who have had much experience in contagious diseases, and some of whom have acquired great skill in diagnosis. These inspectors are, therefore, admirably qualified to teach; and if they are willing to utilize their knowledge in this way, then all difficulties would be solved. If, however, their time is entirely taken up by their present duties, the only alternative is for the Board of Health to allow representatives of the colleges to visit the hospitals and to take classes of students through the wards.

DISCUSSION.

Dr. W. H. Draper.—I think, Mr. President, Dr. Brannan has brought before us a most important subject—one which has interested me for many years, and which must have interested every hospital teacher. We all know that none of our general hospitals are open to patients suffering from any of the infectious diseases except typhoid fever. And we all know how often it happens that the unfortunate victims of scarlet fever, measles, and small-pox have wandered about the town, trying to find some door open for their reception. The only shelter we have for them is that which is under the control of the Board of Health. This has been for years inadequate. It promises now, however, to be so enlarged that it will become one of the most valuable of our public charities. If, in addition to the enlarged accommodation for cases of infectious fevers, the scheme which has been outlined by Dr. Brannan can be carried out, a most important function and a most enlightened duty will be performed by the Board of Health. I suppose, as Dr. Brannan has stated, that a very great majority of those who graduate here in medicine do so without ever having seen a case of typhus, small-pox, scarlet fever, or measles; indeed, I have known men of pretty large experience, men even occupying the position of hospital physicians, who have not recognized a case of small-pox or of scarlet fever. I am willing to confess that in my own experience, which has been a pretty

large one, I have sometimes failed to recognize these cases. It is safe to say, therefore, that a very great majority of medical students never have an opportunity to see examples of the infectious fevers before graduation, and it can hardly be doubted that those occupying positions as dispensary doctors must from sheer lack of experience often allow patients suffering from scarlet fever and from measles to go out of the dispensary with these diseases unrecognized.

It has always seemed to me that every public hospital should exercise the function of a public teacher, and that no man who occupies a hospital position should be allowed to occupy it without teaching. If this condition were attached to hospital appointments, the clinical privileges of New York would become largely more advantageous to the men who come here to study medicine than they are now.

The proposition made by Dr. Brannan to open the fever hospitals under the control of the Board of Health is one which is likely to meet with opposition by the public, on account of the fear which is inspired by aggregating cases of infectious diseases within the limits of the city. That this fear is much exaggerated is evident from the safety with which such institutions are administered, so far as the outside public is concerned, in the large cities of Europe. Considerable reflection, and some effort toward enlisting the sympathy of rich and benevolent people in the establishment of a fever hospital in New York, have convinced me that such an enterprise can only be carried out by the authorities who have charge of the public health. Our Board of Health fully recognizes its duty in this respect, and is all the time enlarging its accommodation and improving its methods for the detection and control of infectious diseases. Dr. Brannan, as I understand, proposes that the Board of Health shall provide, under proper restrictions, clinical privileges for students of medicine desiring to study the objective signs of infectious fevers. It is not unlikely that this proposition will excite considerable opposition. It will be feared that opening the wards of an infectious-fever hospital to medical students will increase the risk of spreading contagion in the community; but when we reflect that cases of contagious diseases in private practice are constantly under the observation of physicians who are engaged in general practice, and that experience shows that doctors seldom

carry infection, this apprehension does not seem to offer a valid objection to this scheme. There are, perhaps, difficulties to be encountered in regard to the grafting of clinical teaching upon the administration of a fever hospital, which will not appear until the time comes to arrange the details of the plan. I believe, however, that these difficulties will be overcome. The Health Board is said to regard the matter favorably and will undoubtedly give the subject deliberate and wise consideration.

One thing is certain, I think, and that is that for a long time to come the public hospitals will afford the only shelter for the victims of infectious diseases who have no homes or who for sanitary reasons have to be taken from them. A private hospital for such cases as resist or dread removal to a public lazaretto is a great desideratum in every large city, but the mere suggestion of such a charity is enough to rouse opposition to it. There is, however, no valid reason why, with the scientific precautions against contagion now in use, our hospitals should not all be provided with accommodations for infectious diseases. Such accommodations are provided in hospitals in England and on the Continent. During a visit to King's College Hospital in London two years ago, I was told that such cases were taken into the general medical wards and that the contagion never spread to the neighboring patients, and only in very rare instances to the nurses. Faith in the principles of Lister, however, has never attained such a height in this country, nor does it seem likely to for a long time to come.

For the present, therefore, we must hail the efforts of the Board of Health for the better accommodation of cases of contagious disease with hearty encouragement and praise; and if, in addition to their services to suffering humanity, they will provide clinical instruction to those medical students who wish to familiarize themselves with the diagnosis and management of infectious fevers, they will deserve the grateful acknowledgment of all who are interested in the cause of medical education and the training of competent doctors.

Dr. G. F. Shrady fully indorsed the views of Dr. Brannan and Dr. Draper as to the necessity for bedside instruction in contagious diseases, and agreed with them in the fact that medical students and young practitioners were entirely without practical instruction in those lines.

He said he had himself been an awful example of a

practitioner who had started out with good, even better than the usual, general preparation, but without a knowledge of contagious diseases.

I was called to see a child on the fourth floor of a tenement house. The mother and grandmother were present. The latter was at the washtub. I saw a child lying in the crib, with a rosy, mottled eruption covering the face and hands, suffusion of the eyes, nasal discharge, and the panting respiration of high temperature. I concluded that there was something the matter with the child, but was entirely at a loss to know what the many signs indicated. Finally the grandmother turned from the washtub, wiped her hands with her apron, and, looking at me askance, with her arms akimbo, asked: "What is the matter with the child?" I pretended not to hear her, being very busy counting the pulse. Finally she repeated the question, and I replied: "Oh, well, you know, a doctor would call that *conjunctivitis*." She looked at me and I looked at her, and I felt that the struggle for victory between doctor and grandmother was getting to be a pretty hard one, when she said: "Is that the doctors' name for *measles?*"

"What did you say?" I asked, wishing still to be noncommittal.

"Is that the doctors' name for measles?" she repeated.

"Oh, of course," I replied, recovering myself with becoming dignity and assumed composure.

That was the first time he had ever seen a case of measles, and his experience had probably been more fortunate than that of many recent graduates. It was, however, a trifle mortifying for him to have the diagnosis of such a common disease made for him by an Irish washerwoman! In conclusion, he remarked that he would, with the sanction of the health authorities, do all he could in his relations to the Willard Parker Hospital to further the efforts of the Academy in bringing about some practical result in this important matter.

DR. H. M. BIGGS.—I have very little to say, and what I have to say is simply as a Fellow of the Academy, and not in any way as a representative of the Health Board. I think there can be no difference of opinion regarding the importance and advisability of establishing such a scheme as Dr. Brannan has outlined for the teaching of contagious diseases. But I think there are many difficulties to be overcome. They do not seem to me to be

insuperable, but I think they are important ones. That it is of very great practical importance in a sanitary sense there can be no question, for any one who has had relations with the Health Department for a short time could not fail to have seen the disastrous results which follow failure to detect cases of contagious diseases. Failure to detect small-pox, for example, has resulted repeatedly in establishing foci of infection, followed by the development of a number of cases. Instances of this sort are constantly occurring, and when the physicians have been summoned before the Board, as they have been this year, they have simply pleaded ignorance, and that plea could only be accepted.

It seems to me Dr. Draper is right in suggesting how the teaching shall be conducted. I do not believe that any physicians should have access to a hospital and its patients, with the privilege of using the latter for clinical or didactic teaching, unless they are also responsible for their care; and it seems to me that the teaching, if such a system were to be inaugurated, must be done by the attending staff of physicians. The medical commissioner of the Health Department has already expressed his opinion that there should be such a staff in connection with the hospitals for contagious diseases—not a staff of consulting physicians, but a staff of visiting or attending physicians who shall be actively engaged in the treatment of contagious diseases, just as are attending physicians in other hospitals with the ordinary medical service. If such a staff could be appointed there would be at once a solution of the question of instruction and as to who should be the instructors. I do not believe that instruction could be carried on in any other way or by any other men than by such a staff. I doubt very much whether this scheme would have been feasible before this year. It would be, I believe, now, with the very largely increased facilities of the Health Department at East 16th street. It certainly would not have been feasible before the establishment of the Willard Parker Hospital, which is only a few years old, and it could only have been developed to a very moderate extent in that hospital until recently. But with the new hospital accommodations which will be at command before the coming autumn, I do not see why, from a purely medical standpoint, not from the standpoint of public policy, a scheme which would be practical could not be mapped out. I certainly hope personally that it can be.

DR. J. WEST ROOSEVELT.—I was very glad to hear Dr. Draper speak so decidedly and, if I may say it, boldly with regard to the advisability, safety, and necessity for having fever accommodations in our general hospitals. This would do more than anything else to solve the question of instruction in contagious diseases. It is, indeed, the only way to get a large mass of fever patients within practical range of the teaching institutions. It is needless for me to say anything in support of Dr. Brannan's position, for his remarks make plain the need of which he speaks. One of the objections which he advanced to his own proposition was that the public at large and the press were likely to regard this matter in a very unfavorable light. I have personal knowledge of the fact that the public and the press are likely to regard the existence of a fever hospital in an unfavorable way, for some years ago there was a considerable sum of money at disposal for building a fever hospital, and the newspapers and some people at that day offered almost furious opposition to the scheme. Their foolish behavior in regard to the matter is now beyond comprehension, yet there is some feeling of the kind in existence to-day. Whose fault is this? Why is it we have this perfectly insane, unreasonable fear and panicky feeling with regard to the contagious diseases? We may just as well face the fact now that it is our own fault and our own teaching; and just as long as the medical profession continues to go on in the course it has been pursuing, just as long as we go through this enormously complex sort of form and ceremony of disinfection, for example, that is now practised, just so long are we going to have that unreasonable feeling.

With the highest possible appreciation of the importance of antisepsis, with the highest possible appreciation of the importance of the destruction of disease germs as far as possible, I think I am right in protesting against the present method of dealing with such cases and of the present method as carried out. I think that the sooner we get the public to appreciate the fact that there is no use for a bad smell in preventing infection; that there is not much danger of spreading typhus; that the district of the London Fever Hospital has been free for years from typhus; that there is no danger of small-pox under moderate care, that no contagious disease is so dangerous as we make it out to be ourselves—the sooner we change our position and get the public to appreciate these

facts, the easier will be our way toward obtaining clinical facilities for instruction in contagious diseases. We have all been through wards—I have myself been through those at the Island—when small-pox, measles, scarlet fever, and chicken-pox were present: the wards were tolerably well ventilated, and there was not much spread of the diseases to other patients.

The practical point which I would make, besides indorsing the need of teaching students, is that we ought to impress upon the public, and I may say upon students too, the fact that we have been talking too violently about the danger of spread of these diseases. There is no necessity for belittling the risks, but it is high time to disabuse the mind of errors.

Dr. E. Kershner.—I do not know that I can add to the information given this evening. I am very glad to hear the reports made here, and I consider the subject a most important one. But, in the first place, I think it is absolutely necessary, before you enforce any such thing as clinical teaching at the bedside of contagious diseases, that we should have hospitals built to which these cases can be taken, and that is one of the most important things brought to our notice. I think it very necessary to have, somewhere in the city, a hospital where all the contagious diseases can be isolated, and I believe, if we are going to stamp out these diseases, to a large extent it will be by such hospitals.

I can agree with the gentlemen who have spoken here about the contagiousness of these diseases being somewhat overrated. I have been in positions on several occasions where diseases have broken out, and have always been lucky enough to circumscribe them and prevent their spread.

As to the necessity of having teaching of this kind, I have experienced the shortcomings spoken of by Dr. Draper and Dr. Shrady. The first case of small-pox I ever saw was aboard ship where there were about fourteen hundred men. I was senior surgeon at the time and had an assistant with me. The assistant knew nothing about small-pox. The eruption appeared and I was forced to rely upon my knowledge to decide what the case was, and, fortunately, I knew enough about small-pox from reading and so on to be able to say that this was a case of that disease; and you can see the importance of a correct diagnosis in the case when the fact is recalled

that a ship is a very close space, with scarcely any circulation of air, making it necessary to isolate the case at once. That was the only case of small-pox which occurred on board at that time.

I remember the same experience with my first case of cerebro-spinal meningitis. I was called one morning to look over thirty or forty sick. There was one fellow who seemed pretty well played out, and I asked the assistant surgeon what he thought of the case. He said he did not know what it was. He was totally undecided. But on looking the case over I could see from the main symptoms that it was one of spinal meningitis, and had the patient isolated. We had no more spinal meningitis on that occasion, although afterward it spread in the neighborhood. I do not know how it got into the ship.

With regard to the cholera the same thing occurred. I had never seen a case of cholera until I was called upon to treat it and protect a crew from the disease. Of course it is very necessary to recognize a disease of that kind from the very beginning. It was in Philadelphia, and I was looking for the cholera at the time, and therefore was better prepared to recognize it when a case came on board the receiving ship. In all these diseases it is very important to recognize them at once and to know what to do—not so much because of the individual patient, but because of others liable to be exposed.

As for burning sulphur and that kind of thing, I consider it almost useless. The way we managed in the case of cholera referred to was to use bichloride and such disinfectants, and carry out perfect cleanliness—everything was kept absolutely clean. In that circumscribed space we managed very quickly to get rid of the cholera.

If we could get in this city two smaller hospitals I think it would be better than one large one. We could receive diphtheria, small-pox, and such diseases, isolate them there and have them treated. I should say it would be a very good thing to have senior students, as suggested, pay to go there and take lessons. At least they could see the cases; they need not stay long. If observant they will soon get to know these cases. If we had hospitals it would do much to prevent the spread of these diseases by making it compulsory to send patients to them at once. Instances occur every day showing the necessity for something of the kind. Recently a gentleman at one of the large hotels was taken with scarlet fever.

The hotel people required him to be taken to a hospital. They wanted to know where to take him. Of course none of the hospitals in the city would receive him and he had to be taken up to North Brother Island. Being a stranger in the city and without friends, he was taken there. I believe he got well. But it ought to be so that a person of that kind at least should be taken to a hospital within the city where his friends, and physicians of his own selection, could have access to him, and also where a knowledge of these diseases could be gained by young men.

Dr. J. W. Brannan.—I am very much obliged to the gentlemen who have taken part in the discussion. There seems to be, practically, agreement as to the necessity and feasibility of carrying out this instruction. The only question is how to do it. I quite agree with Dr. Draper and the other gentlemen who have spoken that we ought to have a fever hospital, or wards in our general hospitals, to which fever cases could be admitted. But we have not them, and, as Dr. Draper said, he has been trying for years to get one of these hospitals, and Dr. Roosevelt has related his experience showing the difficulties to be overcome. We *have* the Willard Parker Hospital. Now, whether the members of the Board of Health, or their subordinates, or college professors shall do the teaching is a question which will have to be decided later. If the students were admitted to the wards to do nothing more than read the label of the disease over the bed and look at the patient, it would be a great object lesson to them. Once we admit the principle that this thing should be done and can be done, I think the question is decided. All the rest is only detail.

The President, Dr. Roosa.—The first case of smallpox which I ever saw was that of a man admitted to the New York Hospital the night before. I did not know what was the matter with the man, but my senior assistant was fortunate enough to have seen a case of smallpox before, and he made the diagnosis. We kept the patient and vaccinated every one in the wards, except one man who had been vaccinated before, and he was the only person who contracted the disease. The knowledge of the disease gained at that time held me in good stead, for one morning in after-years, when my waiting room was full of patients, a young man was admitted who, when his turn came, said he had an eruption and wished me to

send him to a good doctor. I found that he had small-pox. I took care to have him sent to a good doctor, and had all the people who had been in the room with him vaccinated.

Since we have a number of the members of the Board of Health with us and they have expressed their willingness to do what they can in this matter, I feel that some good must come out of it.

Stated Meeting, October 5th, 1893.

MEMORIAL ADDRESS ON THE LATE CHARLES CARROLL LEE, M.D., LL.D.

By HORACE TRACY HANKS, M.D.

TWENTY-FOUR years ago Dr. Charles Carroll Lee was elected a Fellow of this Academy. And during all this time he was ever an active member, and labored for its best interests and advancement. When we look over the list of the fifteen members of the council in 1869, the year of Dr. Lee's election, we find only one remaining with us now, our distinguished ex-President, Dr. S. S. Purple. We also find that nearly one-half of the active members at that time have passed on to their reward. We can well say of Dr. Lee: Thou hast died young, but thy work was well done; "enter thou into the joy of thy Lord." It is pleasant for us to remember that this quarter of a century which Dr. Lee has lived as a Fellow with us has been a very eventful one to the Academy. Dr. Lee was one who urged forward the purchase of the house at 12 West 31st street. He took pride in seeing and aiding in its prosperity while the Academy remained there. He read papers and discussed medical subjects at its sessions and at the meetings of the Obstetrical section. He was deeply interested in seeing this beautiful and commodious building erected. He took a deep interest in post-graduate instruction for medical men, and wrote and taught, and contributed of his means, to this end. In all these twenty-four years much that he did has helped to make medical history in New York City. In every good cause which would advance medical knowledge he stood shoulder to shoulder with our Fellows of blessed memory—our Peaslee, and Flint, and Hamilton;

our Van Buren, Bulkley, and Buck; our Parker, Clark, and Hudson; our Krackowizer, and Sims, Bedford, Budd, Wood, Agnew, Leaming, and White. He worked faithfully for what seemed to him to be the best interest of the Academy, and the medical profession in New York. Such companions, with such high aims, must have strengthened his early ambition to be a respected and honored companion of the noble members of our profession. His friendship for Peaslee, the Taylors, for Sims, Barker, and Van Buren, and a host of those who still live to mourn his death, was real and spontaneous.

It is not always the physician with most thorough education, nor the surgeon with most pronounced skill, who does the most for humanity and leaves a memory most beloved. Dr. Lee was thoroughly educated, and well schooled in that best school, *experience;* and he was a surgeon of acknowledged skill. But how few of those physicians who knew him best admired him *solely* for these most excellent attainments! We knew him as the kind, thoughtful, obliging, entertaining, courtly Christian gentleman. The successful and cultured physician and surgeon was only *one* phase of the many good qualifications which we loved. *Socially* he was charming, and his breadth of culture, even outside that of his chosen profession, made him the superior of most educated men in New York.

Dr. Lee was born in Philadelphia, Pa., March 24th, 1839, and died in his home, New York City, suddenly from pleurisy, May 11th, 1893. He had been much weakened from an attack of blood-poisoning, and from a severe strain of the knee just previous to his sickness. Dr. Lee descended from the distinguished family of Lees which settled in Virginia in 1641. In 1770 one member of this family settled in Maryland. The Honorable Thomas Sim Lee, Governor of Maryland in 1779, was Dr. Lee's grandfather. His father, the Honorable John Lee, married Harriet Carroll, granddaughter of Charles Carroll of Carrollton, the last to die of the signers of the Declara-

tion of Independence. It may thus be seen that a long line of distinguished ancestors had undoubtedly left their impress upon the mind and physique of Dr. Lee. He was graduated from Mount St. Mary's College, Emmittsburg, Md., in 1856, and received his degree of M.D. from the University of Pennsylvania in 1859. His title of LL.D. was conferred upon him by Mount St. Mary's College in 1890. He was successively appointed, after his graduation, to the position of house physician to Wills, Blockley, and Pennsylvania hospitals. He was appointed assistant surgeon in the regular army at the beginning of the Civil War. At the close of the war, after being promoted to full surgeon, he was assigned to duty in New York City on the board of examiners for the medical department of the regular army. Soon after he resigned his commission and settled in this city. He was a warm personal friend of the late Dr. George T. Elliot, and through him and others was at once introduced to the best circle of medical men in the city. He was appointed surgeon to St. Vincent's Hospital and to Charity Hospital soon after he came to New York. From being an assistant surgeon in the Woman's Hospital, under the late Dr. Peaslee, he was appointed surgeon early in 1879, when the staff of the hospital was increased to five surgeons. He held this position for over ten years, when, on account of his laborious private practice, he resigned. He was one of the most untiring and faithful physicians to the New York Infant Asylum. At the time of his death he was consulting physician to the Charity Hospital, St. Elizabeth's Hospital, and the Woman's Hospital. Dr. Lee had often been asked to take a professorship in some of the different medical schools in New York and Philadelphia, but had as often declined the honor. In 1887, however, at the urgent request of the writer, he consented to be elected professor of diseases of women in the New York Post-Graduate Medical School. He held this position at the time of his death, and no professor ever did his duty more faithfully. He was president

of the New York Obstetrical Society for two years. He was vice-president of the New York Academy of Medicine for three years, and at the time of his death he was president of the Medical Society of the County of New York. As a physician or surgeon to a hospital he was exceedingly prompt and faithful. His attention to details and to small courtesies, which really mean so much in his relations with the members of his profession, as well as with his friends, was truly remarkable. During the ten years of my association with him in the Woman's Hospital, if he was to be more than ten minutes late he would send me notice of his detention. He was never absent without informing me that he was to be away. He never proposed to change a course of treatment without first writing it out for the benefit of the house staff, that they might be duly prepared. He took responsibilities bravely; he met emergencies successfully. Although he was not as deft an operator as some, his education, his experience, his hopefulness, and his therapeutical knowledge rendered him at all times a safe surgeon, and his death rate was consequently as small as the smallest. As a clinical teacher he always interested his class with a wonderfully graphic and charming description of the disease or lesion which was present in the patient before him. His influence here was always on the side of honest, helpful instruction. He was ever willing to use new appliances, instruments, and medicines, or to try new surgical operations, when such seemed to be improvements, but never simply because they were *new*. The Christian politeness which makes us all brethren was never forgotten in Dr. Lee's treatment of the poor in the Woman's Hospital and Post-Graduate School. He endeavored to show no difference in his conduct to the rich and the poor. The suffering charity patient from Section 8 was therefore addressed as kindly and treated with as much consideration as were the patients in the private rooms in Section 1. Such habits in his conduct of a clinic at the Post-Graduate School were twice blessed, as the

class was taught the difference between the *gentleman* and the boor. If all young members of our profession who treat charity patients could have seen him, as was his daily custom in the Woman's Hospital, politely ask the least attractive patient from the charity ward to be seated, while he calmed her fears and kindly told her of the character of her disease and informed her what should be done for her relief, it would have been for their profit and their patients' happiness. He never acquired habits, while treating the poor, which had to be forgotten or guarded against while treating the rich.

As a presiding officer Dr. Lee was quick, judicious, and graceful. In this position he showed, *par excellence*, the gentleman of the old school, adorned with all the culture and refinement of the best modern society. Dr. Lee was a hater of shams, deception, and hypocrisy, wherever found. I have often watched his face when some statements were being made which could easily be refuted, or while some plans were being proposed which were altogether visionary, or some instrument was being shown which was quite impracticable. He never wore the look of a man who was about to annihilate the physician who had just spoken, but in a well-rounded and polite sentence he would express his opposite views. His popularity with the profession in this city was remarkable. How few of us could have polled so large a vote at an election as did Dr. Lee when he was elected president of the New York County Medical Society in 1892! At his funeral service in St. Stephen's Church there were more physicians present than I have ever seen at any similar service in this city. It would seem as if every member of the profession felt their personal loss and would testify to it and to their love and respect for him by their attendance at this last, sad rite.

As a host Dr. Lee had few equals, and it could be truly said of him, as it was of Van Buren, that "he could entertain with all the grace and dignity of an archbishop." He was fond of substantial and valuable antique or

modern furniture, and bric-à-brac and curios. He enjoyed an hour at Vantine's or Sypher's in this city with the pleasure of a connoisseur. His home evidenced this cultured taste and love of the beautiful.

As a writer Dr. Lee was busy, and has favored the profession with many practical contributions on important subjects. He wrote the article in the "American System of Gynæcology" on "Diseases of the Vagina." His subjects have been various and have shown his breadth of thought and study. Early in his career in New York he wrote an excellent article on "Hæmatocele." In 1879, in the *Medical Record*, we find his helpful article on "Cystitis." In 1881 appeared an interesting article in pamphlet form entitled "The Treatment of Chronic Perimetritis by Injection of Tincture of Iodine into the Exudate." This was an instructive paper, and helped to establish the belief that such treatment should not be followed. In 1881, in the *Medical Record*, was published his article on "The Proper Limitation of Emmet's Operation." In the Transactions for 1884 of the American Gynæcological Society is found his excellent article on "The Puncture of the Gravid Uterus during Ovariotomy." Later, in the New York *Medical Record*, is his paper on "Puerperal Fever." In 1886 he wrote the very scholarly article, in the "International Encyclopædia of Surgery," on "Ovarian and Uterine Tumors." In 1888, in the *American Journal of Obstetrics*, appeared his paper on "Hysterorrhaphy in the Treatment of Retroflexion of the Womb." At that time this was a contribution which did much to popularize the operation. In the fall of 1891 he read before the New York Obstetrical Society a paper on "The Ultimate Results of the Removal of the Uterine Appendages." This was published in the New York *Journal of Gynæcology and Obstetrics* and in the *University Medical Magazine*. This article was eminently fair and judicious, and no paper ever read by Dr. Lee before the Obstetrical Society gave a better impression of his honesty, his ability, and his judicious mind. In the Transactions of the

American Gynæcological Society and the Medical Society of the County of New York, the Obstetrical Society, and the Academy of Medicine of New York, may be found many pages of most excellent remarks made by Dr. Lee in the discussion of various papers.

Dr. Lee married Helen, daughter of the late Dr. Parrish, of Philadelphia, in 1863, who with five children survive him. One son, Thomas, is in the senior class of the College of Physicians and Surgeons, New York; the other son is a law student in this city. Dr. Lee's domestic life was as peaceful and beautiful as his public life was successful and honorable.

While we feel that we have lost the companionship of an honorable man and one who had the interests of this Academy deeply at heart, we also realize that he had accomplished much; and although dying twenty years earlier than we had hoped or expected from such a vigorous physique, we can appreciate that his was a well-rounded life, beautiful and symmetrical, and filled with good deeds to the end. "We live in *deeds*, not *words;* in actions, not in figures on a dial; we should count time by heart throbs. He lives longest who thinks most, feels the noblest, acts the best."

THERAPEUTIC REFLECTIONS.

BY SIMON BARUCH, M.D.

THE prudent merchant resorts from time to time to an inventory of his obligations and assets, in order that he may diminish the former, and increase or enhance the value of the latter. It would surely inure to the satisfaction of the physician and to the advantage of his suffering clients if he would follow this example. This I propose to do, asking your indulgence if I bring before you trite observations and experiences which are not novel to you. Indeed, if my remarks call forth a responsive echo in your minds, this will demonstrate that I have truly given voice, as is my intent, to actual observations at the bedside. From such I propose to draw lessons and to elicit discussions which may enable some of us to leave this hall better armed for the battle with disease and death which we are daily waging.

Enthusiasm in therapeutics is indeed unwarranted, for the history of medicine abounds in the rise and fall of the most brilliant structures, in the explosion of the most plausible theories, and, what is more painful than all else, the overthrow of remedies which have seemed to be so firmly entrenched in the minds of the foremost physicians for so long a period that they seemed impregnable against all attacks. I need but call from the shadows of the past the spectre of venesection. Though hoary with age, and lauded by men whose works are to-day revered, this therapeutic flower withered under the light of modern physiology and pathology.

Another remedy, once the sheet-anchor in many serious diseases, has been dethroned and has taken the subordinate though most useful position of a simple anodyne.

Time was when in certain forms of peritonitis opium was the chief remedy, because it "acted as a splint to the

inflamed surfaces"; to-day Lawson Tait's teaching, that it is dangerous and that the opposite treatment by saline purgatives is more useful, is most successfully followed. Let him who grows fulsome in his laudations of a remedy bear in mind how these mighty giants have fallen, and endeavor to rescue the remedy he espouses from a similar fate by judicious tests and unprejudiced comparisons. What is the status of therapeutics to-day? What are the assets with which the physician may meet his daily obligations to cure the sick?

Let us confess that the materia medica consists chiefly of medicinal agents whose action on the human body, with the exception of very few, is uncertain, and whose effect upon disease is either neutral or unreliable. How often can we conscientiously say that we have accomplished, by means of any medicinal agent or combination of such agents, the cure or removal of any disease? With the exception of quinine, and perhaps of mercury and the iodides, there is not a single medicinal agent which may be regarded as actually curative. And yet we constantly meet intelligent physicians who insist upon the contrary, and in so doing neglect remedies that are more valuable because their action is clearly explicable upon rational principles. Faith in drugs is almost as frequent among physicians as faith in being drugged among patients. As an illustration I would refer to a case of acute entero-colitis in a child which I saw recently in consultation with a gentleman who is justly esteemed by colleagues and patients. The usual symptoms—fever, a large number of evacuations, loss of sleep and appetite, etc.—were present. Upon the antiseptic theory salol had been frequently administered; to meet other symptoms bismuth, Dover's powders, and other remedies. The simple substitution of intestinal irrigation for the salol, together with an abolition of the milk diet, produced a complete change in this case. It may be held that the latter course is that recognized as proper at the present time. But I cite this case to show that it is

not so universally recognized as it should be, and to illustrate the difference between medicinal and non-medicinal remedies for the purpose of emphasizing the chief point of this paper, viz., the too frequent neglect of those non-medicinal agents whose action in disease is so simple, rational, and clinically effective, and the unhappy reliance on pharmaceutical preparations whose effect, like that of salol, as an intestinal antiseptic, has never been definitely proven. The vast majority of remedies in our pharmacopœia have no better foundation than salol, and yet they are constantly used upon the strength of so-called clinical observation.

The rock upon which therapeutics has always split, and which continues to menace us because we seem not to profit by the teachings of history, is the treatment of symptoms. In my student days, when venesection was in its last gasp, veratrum viride had in the South gained many adherents. A rapid pulse was the chief manifestation of all fevers; here was a remedy which in small doses perfectly controlled the pulse. A reduction to one-half of its rate could readily be produced. I shall never forget one of the first cases of pneumonia which I treated as house physician of a hospital.

Veratrum was given until the pulse was reduced from 140 to 80, and yet the consolidation continued to the bitter end, and the patient succumbed with a normal pulse. History repeats itself in the application of the coal-tar antipyretics recently so much in vogue.

The temperature may be readily reduced from 105° to 100°, and, as in the case of veratrum, we may even have the questionable satisfaction of seeing the patient die with a normal temperature. This striving after symptomatic remedies is the unhappy legacy which our forefathers have bequeathed to us. It is an echo of this mysticism which veiled their idea of disease and which led them to regard the latter as an essence rather than as a manifestation of certain disturbances in the economy which rendered the latter incapable of regulating its

functions. Therapeutics would be a simple art in this day of patent medicinal agents, could its aims be merely filled by meeting certain symptoms or certain definite alterations in the functions of the body.

As pathologists it is our function to discover changes in the organs; but as physicians who stand at the bedside of those who implore us to heal them, we can accomplish but little if we are called to act after these changes in the tissues have taken place. As has been well said by another, the physician who is called to combat a case of well-established organic disease with remedies may be likened to the sanitarian who is called upon to prevent an infectious disease by disinfecting the emanation of the patient. The physician who would cure must assume the task early—in the beginning of that anomalous relation between capacity of the system for work and the demand upon it for work which is really the inception of all tissue changes, and which may be prevented by limiting the labor imposed upon the organs. To discover this anomalous relation in its incipiency, and to guard against its development into organic disease, is the function of the true physician. The latter must soon discover that in this contest medicinal agents play an unimportant rôle when compared with those methods which we learn by observation of Nature's course in health. As an illustration may be mentioned dilatation of the stomach, an organic alteration which is most commonly the result of long-continued insufficiency of its functions. The early recognition of the causes, the adaptation of diet, mode of life, manner of eating, gastric lavage, etc., to each individual case, will conduce far more to the prevention and cure of the organic disturbance than all the medicinal agents, from ancient gentian to modern strychnine, from muriatic acid to pepsin. No physician of experience can gainsay this proposition. It must be patent to the most enthusiastic polypharmacist that he accomplishes only an amelioration or removal of certain manifestations of disease—a result which may, and probably does, facilitate a

restoration to health, but is rarely, if ever, the direct means of such restoration.

In the large preponderance of acute diseases, among which may be numbered all infectious diseases, the administration of these medicinal agents is not only entirely symptomatic, but in many instances absolutely harmful. Take as an example a case of appendicitis. The tendency to treat such a case as localized peritonitis and to meet the symptoms tempts the average practitioner to prescribe morphia to subdue pain, ice or poultices to the right iliac regions, food to satisfy the bugaboo of coming failure of the vital powers, and an antipyretic to reduce the temperature. This is not a fancy picture, but a chapter from personal experience. Coming into contact with such a case as a consultant, how can the latter take his bearings? Like a mariner in a fog, he is handicapped; pain is absent, temperature perhaps normal, all is serene except the anxious countenance, perhaps vomiting, and the attendant's fear that something may be brewing.

Here the remedies employed have hidden the enemy's movements; the physician has become the ally of the disease; diagnosis and prognosis become difficult, and valuable time may be lost. I am sure many of my hearers have passed through this experience. Am I presuming too much, then, if I counsel that in the early stage of disease the warning of Italy's lamented clinician, Cantani, be constantly before our eyes, "Nil nocero"? To make a clear diagnosis is important for a favorable issue. It is better for the patient to suffer some discomfort than to have his life jeopardized by remedies which obscure the real issues of the case. Later in the case, too, it should be our constant endeavor to avoid damaging the system which is staggering under disease and cries out for every possible aid on the part of the physician. In this day of powerful symptomatic remedies, veratrum for the pulse, antipyretics for the temperature, pilocarpine for the secretions, morphia for the pain, chloral for sleeplessness, all agents which strike mighty blows at the manifestations of

disease often lead to disaster by lulling the physician into a false sense of security. And yet how eagerly they are resorted to daily observation teaches but too sadly. It may be urged in defence of such a course that the patient and his friends are importunate to have the pain, the fever, etc., combated. Medicine is a business, said a gentleman in a recent discussion on antipyretics, and we must yield to the demands of our patient to be relieved of his fever. It has doubtless been your experience, as it has been mine, that much can be accomplished by placebo, by mild remedies, by gentle reassurance, and by taking the friends into our confidence.

When there is severe pain, as in some cases of appendicitis, I prefer to keep the patient gently under the influence of chloroform until a decision for or against operative measures can be reached. In a case of perforating appendicitis at Long Branch two years ago morphia administered by an able colleague was discontinued, chloroform substituted for twelve hours with intervals, until Dr. Wyeth arrived. The patient was not aware of his arrival, nor of the laparotomy performed on her, until an hour afterward. She made a good recovery. Ten years ago the belief had become all but unanimous that the only direction in which therapeutic effort was most conducive to the patient's benefit lay in following the tendency of Nature in each disease. *Vis medicatrix naturæ* is an old rallying cry of the profession, but one which did not gather many steadfast defenders. At one time a complete therapeutic Nihilism, emanating from the Vienna school, threatened to become its expression. But when the chemist evolved those potent therapeutic sledgehammers which could beat down symptoms, the gentle handmaiden Nature was thrust aside and the patient was beaten down with the symptoms. To-day a reaction is happily setting in. Once more are heard warning voices that bid us follow Nature's prompting. To join these and to re-echo them in these halls is the purport of my paper to-night. For instance, rest is the first prompting

of Nature in diseased conditions. What can be more simple and obvious than rest in inflammatory conditions? The surgeon puts an inflamed joint into a splint and trusts to Nature—*i.e.*, the restorative powers of the system—to heal the disease. May not an acutely inflamed kidney be treated in a similar way? Its functions must go on; in this respect it differs from the inflamed joint. But the intelligent physician, knowing that the chief function of the kidney is to eliminate urea, so arranges the patient's diet that this work is reduced to a minimum; knowing that other functions of the kidney may be vicariously taken up by the skin and bowels, he endeavors to utilize these vicarious eliminants for the purpose of relieving the inflamed organ from labor. And yet we find the contrary so often that I may be pardoned for emphasizing here the necessity, nay the duty, of doing in internal diseases what is so obvious and so successfully done in external diseases. Only recently I saw a case of acute nephritis with a well-informed physician of ten years' graduation from a most exacting school, a hospital physician too, who treated the case with digitalis and acetate of potassium as diuretics, because the secretion was reduced to nineteen and a half ounces, paying no attention to the physiological aids which Nature points out.

In acute conditions rest must be provided; in chronic conditions rest, judiciously alternated with gentle exercise of the functions of the diseased organs, is indicated, and will prove far more useful than medication. Let me not be understood, however, as despising the latter. There are few to whom I should be willing to yield in dependence upon our well-tried medicinal agents. But I spurn them if their action is based on empirical observation alone, the *ipse dixit* of one or more men.

Those medicines alone which evince their presence in the system by certain definite signs have my confidence. When quinine is administered in sufficient doses to cinchonize the patient, I know, without fear of contradiction, that whatsoever effect is produced on the paroxysm may

be attributed to it; when it is used in small doses three times a day, for so-called malaria, I spurn the remedy as I do the disease. True malaria will as surely yield to cinchonization before the paroxysm as spurious malaria (the so-called civic or international malaria) will fail to yield to it. This much I may say positively, from a long experience on the banks of Southern rivers.

To refer to the value of salicylic acid in acute articular rheumatism would seem supererogation in this assemblage. But bear with me if I apply it as an illustration of my views on positive medications, for which I plead. I have seen capable men in hospital and private practice order five grains of salicylate of soda every two hours in rheumatism. Is it surprising that these gentlemen are unwilling to testify to the value of this remedy? Twenty grains every two hours of pure salicylic acid until tinnitus aurium is produced and affords evidence of its having been absorbed, is the method I adopt. I can therefore not be charged with antagonism to reliable medicinal agents. I would only inveigh against the thousand-and-one impotent drugs with which the sick are deluged—drugs without ascertainable effects upon the economy, though many frogs and dogs may have died and lived under their experimental use, have no charms for me. If careful observation among various classes of patients in country and city, private and hospital practice, in peace and war, may crystallize one's experience into some definite shape, permit me to say here to-night that far more actual curative results may be obtained from watching and cautiously following the tendencies of a disease and applying those agents which are potent in maintaining the system in health—viz., rest, exercise, diet, baths, change of environment—than from the most intelligent application of medicinal agents, highly though I esteem some of these. To demonstrate this proposition in a representative acute disease, let us choose typhoid fever. Here we have a malady which runs a more or less definite course, whose etiological factors are beyond our control. We are called

upon here to contend against a foe of great power. The system is overwhelmed by the toxines due to the biochemic activity resulting from the life or death of the micro-organism to which the disease is traceable. Be that as it may, we do know clinically that we stand in the presence of an intense toxæmia which exercises a depressing, a devitalizing influence upon the nervous system. Every function which draws its force from the latter suffers visibly. The brain itself is more or less overwhelmed.

The patient lies in a semi-stupor, his face is expressionless, the eyes dull and glassy, lids have closed (typhoid countenance). His perceptions are blunted, he mutters incoherently, he has no control over his sphincters. In brief, there exists great cerebral depression.

The pulse is rapid, feeble, dicrotic—all manifestations of cardiac enfeeblement. The blood is deteriorated, its leucocytes decreased. The respiration is shallow and ineffective; hypostatic conditions, bronchial congestions, and pneumonia result. The temperature is elevated, either because of the loss of regulatory influence of the heat centres, or by diminution of heat radiation from the skin, or both. The secretions are impaired; the urine, for example, is concentrated, highly acid, and loaded with products of active tissue change. These, together with the shallow respiration, are evidences of the crippled condition of the emunctories, whose integrity is vitally important for the maintenance of health. In brief, the patient's vitality is sapped because the organs upon which it depends receive imperfect sustenance from the nervous system. Modern physicians are agreed that every effort should be made to maintain the patient's vitality, to counteract the palpable toxæmia, be it due to micro-organism or not—in short, to sustain life until the disease has run its course. The expectant treatment, so-called, was the outcome of the fatality of the former spoliative treatment. Liebermeister's great success in the Bâsle Hospital made high temperature the chief point of attack.

So long as we had no real medicinal antipyretic it was not clear whether to the (medicinal) quinine or to the (physiological) baths administered by him were due the superior results which he attained. The high-temperature idea was plausible and gained many adherents rapidly, until the discovery of the coal-tar series brought to the physician the first real antithermic remedies. A few years' use of these positive remedies has awakened the profession to the fallacy of regarding high temperature as the chief lethal agent. To-day a reaction is impending. While in the medical centres the coal-tar antipyretics are being reluctantly abandoned, it will be long ere the less enlightened rural practitioner will let this comforting drug slip from his fond grasp. The same fate has befallen other medicinal agents, which, however, were not so readily abandoned, because their specific effects are not so patent; for example, veratrum, to which reference has been already made. To-day internal antisepsis has become a fad. Though claiming to be based upon the attractive germ theory, which threatens to upset all past pathology, the advocates of this method have thus far failed to demonstrate its tenability by actual experiment. Until such demonstration shall be made, it is difficult to accept the idea that the mild antiseptics (naphtholin, naphthol, sulpho-carbonate of zinc, etc.) which are applied in typhoid fever are capable of disinfecting a long intestinal canal containing more or less fluid, which dilutes or otherwise neutralizes them. Moreover, long before the disease has been diagnosed, during its incubation, these pathogenic germs have been circulating in the blood. Having already passed into the lymphatic glands and spleen, it borders on the absurd to assume that these antiseptics are capable of searching out and destroying them. When the patient comes under observation he is really suffering more (if deductions from recent investigations may be accepted) from the toxines generated by these pathogenic bacilli than from the presence of the latter. Disinfecting

the intestinal canal may prevent some auto-intoxication, but it surely cannot reach the pathogenic germ nor its toxines.

That there are fewer bacteria in the fæces after some of these antiseptics are administered, even if it had ever been positively demonstrated, does not by any means justify the deduction that the intestinal mucous membrane has been rendered aseptic, much less so than that pathogenic germs have been destroyed. The surgeon would not be content with such a demonstration. Why should the physician be? Hence this pseudo-scientific claimant to our faith in numerous drugs is feeble indeed, and must go the way of its predecessors.

It follows from these brief considerations, into which the lack of time does not permit me to enter more fully, that the only active medicinal agents, those agents whose effect has been positively demonstrated, have simply afforded the physician means for suppressing certain pronounced symptoms. Pulse and temperature are still important indices to the condition of the patient. Nevertheless, despite the fact that we have positive medicinal agents to bring them to an almost normal standard, they are proven absolutely harmful by good observers. The physician, therefore, is forced to acknowledge his entire dependence on the *vix medicatrix*, which he attempts to aid by withholding all injurious elements and utilizing physiological remedies.

Rest, seclusion, proper adaptation of food and drink, and cleanliness are the chief agents in the so-called expectant plan of treatment, whose superiority over the former spoliative and the present antipyretic treatment clinical observation has demonstrated. There is, however, one physiological remedy which, judiciously adapted to the case as are diet, rest, etc., has produced a vast improvement in the condition of the patient and in the final issue of the case. The bath treatment of typhoid fever no longer requires defence before an enlightened medical audience. It has demonstrated its value in so many clini-

cal texts on large numbers that I need refer here only to its salient advantages. Being a physiological agent—*i.e.*, an agent if used in health, promotes the latter—its judicious application is as potent as that of air, food, and rest. Its injudicious application is as injurious as that of the latter agents. For example, the wrapping of a typhoid patient in a cold, wet sheet, and sprinkling it with ice water until the temperature is reduced—a method actually used by eminent practitioners—is as injurious to the patient as the exposure to draughts of cold air or the administration of a mixed diet would be. In the one case it would still be bathing, in the other it would still be ventilating and feeding. In all it would be an injudicious, senseless application of useful remedial agents.

The judicious application of ventilation and diet is pretty well understood by the profession; the judicious application of water is unfortunately still a *terra incognita* to the large majority. It is true nearly all utilize water in some form. Cold sponging is quite commonly adopted in fever, and yet the feeble refreshment obtained from it may be compared to the ventilation obtained by the opening of a window an inch. Injudicious application may assume both extremes of directions, doing too much or too little.

The Brand bath has proved the most successful method of treating typhoid fever. The more nearly we approximate it the more surely may we depend upon obtaining its grand therapeutic results. This has been acknowledged by Juergensen and others, and it is the personal experience of many, myself included.

In a letter received a few days ago from the venerable author of the Brand method, he writes: "Who reads what we write? Who acts upon it? No one. My typhoid treatment has proved this sufficiently now. After thirty years' labor, debate, discussion, and polemics, only a few know, the multitude knows not, what the aim of the Brand method is; they still confound it with the symptomatic method." These words from the man who has

revolutionized typhoid treatment are indeed a sad commentary. They truthfully depict the present state of knowledge on this subject. In this city there are only three hospitals in which the Brand method is correctly applied; in others it is modified to suit personal predilections, irrespective of the fact that its results cannot be any more fairly judged by such modification than the rules of antisepsis in surgery can be judged if some essential of cleanliness be omitted.

The rationale of the bath is so simple that it appeals to our best judgment at once. We have an overwhelming of the nerve centres by the products of infection. The shock and subsequent stimulus to the cutaneous surfaces are conveyed to the nerve centres, and thence reflected to the heart, lungs, and other organs. Observation at the bedside at once renders these effects patent. The first effect is a refreshment, an enlivenment of the cerebrum. The eyes are opened; the face loses its apathetic stare; consciousness returns after one or more baths; the inspiration is deepened; expectoration is facilitated; the widening of the peripheral vessels and the stimulation of their coats relieve the heart; blood pressure is increased, and the laboring organ becomes as quiet as does a sea-tossed ship in the hands of a skilled mariner. The secreting glands are aroused to activity. Moreover, the temperature is reduced, not so violently as by medicinal agents, but more definitely, more in accord with normal tendencies. In brief, all the manifestations of the disease are favorably influenced, because the normal standard is slowly but steadily and lastingly approximated under the influence of repeated judicious bathing. Even the exacting demands of the most recent ideas are met by this treatment. Mentchnikoff has shown, by his interesting studies, that inflammation is the phagocytic reaction of the organism to an irritant. Cells are phagocytes, hungry to devour any toxine or microbe that may find entrance into the blood. We may successfully aid the system in this "reaction against toxines" by endowing its

main vitalizing agent, the nerve centres, with vigor, by furthering elimination from the skin and kidneys, by removing hyperæmia of the organs and facilitating the passage of phagocytes into the tissues, but more especially by rendering the blood more alkaline and thus more favorable to the phagocytes.

All these are accomplished, according to well-established experiments, by the cold-bath treatment. It has been recently shown by Thayer, in the Johns Hopkins laboratory, that the leucocytes are more than doubled after a Brand bath. On the other hand, the destructive effect of medicinal antipyretics on the leucocytes, their inhibition of excretion of products of tissue change, and their effect on the heart have been again and again demonstrated.

Here we have an obvious illustration of the action of medicinal remedies and physiological remedies in a very common acute disease. The same observation may be made in all acute diseases. While in those of short duration, and characterized by absence of serious depreciating tendencies, medicinal agents are more useful chiefly because more agreeable and practicable than baths, the latter are far superior in all those acute maladies which endanger life. Baths, though unpleasant to the patient and friends, absolutely give sustenance to the crippled organs which are struggling to maintain life; while medicinal agents, though agreeable to patient and friends, serve only to enfeeble the heart, cripple the kidneys, and thus aid the disease rather than the patient.

In chronic diseases the difference in effect of medicinal and physiological remedies is no less striking. If such diseases are attacked ere serious organic degeneration has ensued, the most threatening and subsequently incurable diseases become amenable to treatment. Even when these are advanced, good results may be expected. I can affirm from personal observation the statement of the great Neapolitan clinician, Semmola, who says: "The methodical internal and external application of water,

modified in temperature as required, together with climate, an appropriate diet, muscular exercise, etc., are aids by which hydrotherapy excites cutaneous activity and with it all functions of tissue changes and organic purification, so that by these means often real marvels of restoration in severe and desperate cases may be accomplished." The reports of the Montefiore Home for Incurables, and my papers read before the State and County Medical Societies, contain clinical proofs of the correctness of this apparently enthusiastic view. Cases of intractable anæmia, dyspepsia, angina pectoris, Bright's disease, diabetes, phthisis, and epilepsy are recorded which have been under observation by Drs. Gaillard Thomas, Janeway, A. H. Smith, Jacoby, and others, the results of which justify Prof. Semmola's claim.

It is not my purpose, however, so much to dwell upon these results as to afford illustrations of the superiority of these hygienic or physiological measures over the medicinal agents. Let us take as an example, for this purpose, chlorosis. I have chosen this chronic affection because it is as well or perhaps as poorly understood as any other, but more especially because, in its treatment, one medicinal agent, iron, has long been regarded as almost a specific. Chronic diseases do not, like the acute forms, tend to spontaneous recovery—chiefly because patients affected by them are not, as they are in acute diseases, forced to be removed from their etiological agencies, and they are not, as in acute disease, usually subjected to physiological remedies. A patient suffering from typhoid fever or scarlatina is withdrawn from the source of his infection; he is placed at rest and secluded; good air, proper diet, etc., are usually afforded him as a matter of course. A young woman suffering from anæmia usually remains within the sphere of the etiological factor of her disease, while the doctor attempts, by iron, food, etc., to reconstruct the blood, which is unhappily regarded as the chief point of attack.

There can be no doubt in the mind of any practical

observer that the same care that is given (perforce) in typhoid fever to the removal of the patient from his unfavorable environment would, if afforded the chlorotic girl, conduce far more to her recovery than the faithful dosing with iron in any of its more or less vaunted forms. Who has not seen a pallid school-girl, who had been for months dosed in vain with Blaud pills, regain her ruddy color, spirits, and appetite so soon as she was removed to the country, where she had access to unpolluted air, water, and food, where she was allowed untrammelled movement of muscle and lung and heart? Hygienic remedies are as potent for good here as they are in typhoid fever. Many of the latter recover under the expectant or medicinal treatment, more recover under a rational application of physiological remedies; many cases of chlorosis recover at home, even under unfavorable conditions and unphysiological treatment, more recover under the judicious use of exercise, rest (which is just as important), proper (not necessarily so-called strengthening) food, etc. Despite these patent facts, physicians continue to ply these unhappy patients with drugs whose reputation rests upon a purely empirical basis. The abuse of iron in anæmia is a crying shame, when it is considered that the quantity in the entire human body amounts to about fifteen to forty-five grains, and that the greatest loss ever found in an anæmic patient was three to four grains, which one-quarter pound of good beef would readily furnish. How much more rational is the removal of the patient into pure air, where the hunger of the oxygen-carrying hæmoglobin may be appeased! How much more rational than iron administration is the placing of an anæmic seamstress (?) into a well-aired hospital ward, where good food, rest for her treadle-wearied limbs, baths, etc., could be methodically applied!

Allow me to urge a more methodical application of non-medicinal remedies. If there is any point upon which we are derelict as a class, it is the lack of precision in our prescription of remedial measures, especially those

valuable non-medicinal agencies which, as Nothnagel and more recently Osler have shown, are coming to the front as chief remedies. While there is no lack of insistence upon the systematic administration of medicines, while the regulation teaspoonful doses three times daily, or powder every three hours, are swallowed with the utmost punctuality, the patient suffering from a chronic malady is allowed to drift along as best he may, taking his exercise, his diet, his rest, his bath, as pleases his sweet will or suits his personal convenience.

In this connection I cannot refrain from again referring to the neglect of the chief hygienic remedy in chronic disease—water. Its value in acute diseases is so recognized by some of the best teachers in this city and country that I may well leave its fate to the good seed annually sown by these earnest men. In chronic diseases, however, which are the bane of our lives and the opprobria of medicine, much missionary work is required to arouse a just appreciation of the great value of water as an auxiliary to other physiological remedies. Abroad this is no longer the case. The most prominent teachers and consultants constantly dwell upon and prescribe it. In this country a few men like Draper and Peterson have written upon it. The clinical results obtained by it abroad and at home surely entitle it to a more careful investigation. While electricity, whose clinical results are far inferior, is taught in our schools, there is not a single chair from which the principles of hydrotherapy are explained or its technique demonstrated. If one tithe of the interest manifested in new drugs, which in most instances benefit only the manufacturer and leave physician and patient deluded and disappointed, were devoted to the study and practice of hydrotherapy, our knowledge of this subject would be enriched and suffering humanity would rise up and call us blessed.

If I have referred more freely to the application of water in this connection, my reason lies in the fact that it is the most powerful agent among physiological remedies,

both for evil and for good, and the least understood and appreciated by the profession. The extract from Dr. Brand's letter given above, and the rare application of hydrotherapy in chronic diseases, testify to the truth of this proposition.

Though the modern physician claims to have emancipated himself from dogma and the schools, he is enslaved by the drug-prescribing habit. Drugs are to-day presented in alluring forms and rapid succession; they come to us, not with the sounding of trumpets, but with the insidious claims of a few clinical tests of Professor So-and-so and Privat-Docent So-and-So. Often we are not even vouchsafed information as to their method of manufacture. If their composition is given it is in the new chemical nomenclature, which to most of us is absurdly incomprehensible. For instance, agathin, a new remedy extolled for neuralgia, is said to be the salicyl-alpha-methyl-phenyl-hydrazone; it is obtained by the interaction of salicylic-aldehyde and alpha-methyl-phenyl-hydrazone.

I plead to-night, not for the abolition of drugs, some of which I value as highly as any one, but for a more methodical application of remedies like rest, exercise, diet, water, and proper environment—remedies which boast of greater antiquity than any medicinal agent, remedies which have demonstrated their value in all epochs of medical history and which to-day challenge the most searching investigation.

DISCUSSION.

DR. A. L. LOOMIS said it seemed to him that the paper was an argument for the use of common-sense in the treatment of disease. He believed that every good physician, every careful observer, every intelligent man, as he grew older, became less and less attached to drugs. The more he saw of disease and the more he treated it, the more did he become convinced that the most reliable therapeutic agents were proper food, fresh air, exercise, rest, and water. The speaker was sometimes astonished,

on carrying a patient safely through typhoid fever or some other form of acute disease, at the few medicines he had prescribed. About three years ago he read a paper before the State Medical Society in which he thought he had shown conclusively that ordinary use of antipyretics did harm rather than good. It seemed to him that when we studied the condition of our patient, the causes of his disease, and their immediate and ultimate effects, we would treat him rather than the disease, using, for the most part, such physiological remedies as the author had referred to. He had sometimes thought that Dr. Baruch had gone a little "crazy" on the use of water in his treatment of disease. Perhaps he was himself a little off on fresh air, climate, change of surroundings, and all that these imply. If we are enthusiasts the tendency is to favor one line of therapeutics more than another, however good or bad it may be; but the statements contained in the paper in regard to the treatment of disease by drugs he most heartily indorsed.

DR. A. JACOBI.—I have carefully listened to Dr. Baruch's paper, and, while I freely admit the value of the physiological remedies he has mentioned, I also advocate the use of drugs. No sensible person to-day claims that the entire science of medicine or therapeutics is prescription-writing, no more than is diagnosis, which, forty or fifty years ago, was in Europe considered the only point of importance. Both are necessary. We, as individual physicians, have to deal with individual cases. Many, perhaps most, individual cases have a tendency to get well. If the patient be kept in bed and supplied with fresh air and such diet as is appropriate in the different stages of his disease, he has a good chance to recover. Such a case is only disturbed by unnecessary medicines. Now and then, however, medicines will certainly be required. For instance, in a case with fever and disturbance of the gastric secretions, it is better to give a few doses of hydrochloric acid than to go on without it. The use of the acid corrects a symptom—a symptom of gastric incompetency dependent on insufficient circulation due to the pyrexia. In most instances, however, the less medicine we give in a case that exhibits a tendency to run its course and recover, the better. On the other hand, we are not, as a rule, mere public sanitarians, but are obliged to deal with an individual case, which we must treat on its merits. It is all very well to quote statistics of one

thousand cases of pneumonia that have been treated after a certain fashion. Those statistics do not teach us anything about the individual cases. During the treatment of an individual case necessities may arise that will take away the odium from all the remedies that have been proscribed (not prescribed) to-night. For instance, venesection has been condemned *in toto* this very night in the treatment of pneumonia. There are, however, cases of pneumonia wherein you have to deal with extensive consolidation, with great cyanosis, with cold perspiration, with a weak pulse, and in such a case venesection is the only resource that will save the life of your patient. I do not say that venesection is *the* remedy in pneumonia, but I do say that it may be required in a case where the right heart is so much overfilled with blood that the circulation cannot go on, and in such a case blood-letting is a blessing, and not in disguise. There are cases of pneumonia in which venesection is early indicated, and in such cases it is a physiological remedy. We have been told to-night that the physiological remedies are only the dietetic ones —food, water, hygiene, etc.—but I claim that in such a case, where venesection is employed to relieve an overfilled right heart, it is the very physiological remedy.

In everything the doctor has said about over-dosing and about the impropriety of inconsiderately treating a high temperature, I absolutely agree with him. Still, occasions arise when we must use antipyretics. You may have a case with very high temperature where water cannot be satisfactorily applied, and where it is absolutely necessary to reduce the temperature in order to allow the stomach to absorb food. The stomach, under the influence of a temperature of 105°, 106°, or 107°, will not absorb, and in such a case I do not see why antipyretics should not be used. It will not do to say that water will answer the same purpose; it may answer the same purpose, but it will depend on the common-sense and experience of the individual physician whether in that case he can employ water, or whether it is necessary to immediately give an antipyretic. Such cases I have seen, such cases Dr. Baruch has seen, and all those who meet cases in different circumstances, rich and poor, strong and feeble, in all the comforts of a well-equipped dwelling and in the fifth story of a tenement house, without a bath-room within ten blocks.

I agree with Dr. Loomis that the older we get the

more we are liable to drop medicines. That is all very well. In this respect we pass through the same revolution that the Pharmacopœia does, in which the number of remedies are gradually dwindling down in successive editions. The number of remedies in the German Pharmacopœia has been decreased by two hundred since the last edition. That is what we are doing in our work, but I hope I shall not live to drop all the remedies excepting two or three. I want salicylic acid (which perhaps I do not give in such large doses as Dr. Baruch does), I want quinine and a great many others, particularly heart stimulants. Many diseases must be treated by drugs in order to keep the patients alive, and they will not get well if you depend only on water or air. I know that diphtheria should be treated in the early stage before absorption has taken place. I know that rheumatism should receive early treatment, in the way the doctor has indicated, in order to prevent an endocarditis. Unless you treat it you will expose the patient to a longer duration of the disease, and perhaps to other complications. I never permit myself to let a case of whooping cough alone simply because I know that in three or four months whooping cough will run its course. I know that if I can reduce the duration of the disease by even two weeks I lessen the danger of dangerous complications by that period of time. Whether I give my atropine, or you give your chloral or some other drug that is known to have an effect on the duration of the disease, I think it should not be omitted. So there are many diseases in which we must employ drugs as remedies. The few I alluded to may suffice.

DR. G. L. PEABODY.—I have listened with a great deal of interest, as we all have, to Dr. Baruch's paper and the remarks it has called forth. In the main I agree with the general drift of his communication, but in certain details I find myself somewhat at variance with him. There can be no doubt that it is far better to resort to some such rational therapeutics as he has indicated, rather than to symptomatic therapeutics, which he has in such a wholesale way bowled over. Symptomatic therapeutics is often no therapeutics at all; in a large proportion of cases it is simply a misnomer. Occasionally, however, symptoms arise that must be treated merely as symptoms.

When we come to Dr. Baruch's treatment of chronic diseases—anæmia, for instance, which he would treat by

sending pale-faced maidens to the mountains or seaside—I agree with him that this is good treatment if they have money to pay their way; but we often see patients who *must* be treated in the environment in which we find them, and in such instances it is a mistake to condemn drug treatment. I believe in the administration of iron in chlorosis. I also believe in intestinal antisepsis. I do not believe that intestinal antiseptics will ferret out the bacteria of typhoid fever and annihilate them at a blow, but I think that they can prevent intestinal fermentation and promote intestinal digestion. In this connection I do not refer to typhoid fever, in which I do not think intestinal antiseptics are of much value, but to intestinal indigestion, of which I think chlorosis is a pronounced example. Cases are accumulating to show that intestinal antisepsis is likely to do good. Forchheimer, of Cincinnati, at the meeting, last spring, of the Association of American Physicians in Washington, read a most instructive paper, in which he showed, not only clinically, but experimentally by the results of his work in the laboratory, that intestinal antisepsis did aid in the formation of blood in animals that had been rendered anæmic experimentally, and in young women that had been made anæmic through the exigencies of their life.

With regard to blood-letting, I understood Dr. Baruch to condemn it in all respects. I cannot agree with him in this. I am quite as sure as one can be that he has saved life in a condition of impending death, that I have saved life by blood-letting in cases of poisoning by coal gas. I am also quite sure that I have seen marked benefit result from fairly profuse bleeding in aggravated cases of pneumonia, such as Dr. Jacobi has described. Of course the antique method of bleeding almost to syncope has disappeared and should never be revived, and the practice of bleeding people in the spring of the year in order to cause them to remain well has also disappeared; but in certain cases of pneumonia, of sunstroke, and of poisoning by coal gas I look upon blood-letting as the sheet-anchor.

Of course I know very well that Dr. Baruch did not go, in his paper, as fully as he would be willing to go into the subject of cold baths in typhoid fever. On this subject I agree with him perfectly. I think that all other methods of treating typhoid fever fade into insignificance as compared with systematic cold baths. Unfortunately, again, there are some patients who cannot afford them.

To pursue this treatment systematically in a private house necessitates relays of nurses, and this makes it exceedingly expensive. A former house physician of the New York Hospital, Dr. Conner, originated a device to which he gave the name of bed-bath, and which is a fair substitute for the tub-bath, although by no means equal to it. I have found it a valuable aid in treating typhoid fever. The bed should be a spring bed, so that it can be readily depressed, the body of the patient lying in a hollow. The bed is covered by a rubber sheet, the sides of which are elevated by rolled blankets, so that the patient lies in a fairly deep puddle of water. In order to keep the water at a temperature of 65° F., ice must be put into it; otherwise its temperature would be speedily raised by the temperature of the patient. The water is then freely splashed over the patient and friction is practised. While this is inferior to the tub-bath, it is better than the cold pack or sprinkling with water, and it can be accomplished by a single nurse. I have employed it in many cases with apparently good results, and am now using it in three cases with satisfaction.

In the practice of medicine it is, of course, of paramount importance that we should make a correct diagnosis. I think the symptomatic treatment of disease, as it is called, originated largely in ignorance. The doctor does not know what is the matter with the patient, and he simply treats symptoms. I think it is a great misfortune that the American people do not demand good doctors, and it is the fault of the public that we have such a large supply of poor doctors. The people, as a rule, want constant medical attendance, for which they often pay ridiculously small fees. I do not think it is as much a question of medical education as it is education of the public. We doctors are called into being merely because there is a demand for us. Until the public wants a better doctor and is willing to pay for him, it is likely to continue to get what it receives at present. The demand as I see it, especially in the rural districts, is for a large supply of poor doctors, and until this is changed it is hoping for too much to expect the supply to be materially improved.

DR. S. BARUCH.—The statement made by one of the gentlemen, that as we grow older we use less drugs, is precisely the reason why I have reached that conclusion. Thirty years ago, when in consultation with an intelligent old physician in Winnsboro, South Carolina, he told me

that after I had been in practice my faith in drugs would diminish. Although I then dissented from this view, I acknowledge its correctness to-night. I ask that students be taught these things, and I think Dr. Loomis knows of at least one college where drugs are much more taught than common-sense. If I am crazy on water, then there are a good many lunatics in Europe, and one such lunatic has spoken quite rationally to-night.

I did not condemn venesection in pneumonia; I simply gave the general opinion of the profession on the subject, to illustrate how a remedy once considered great has fallen into obscurity. I think that I, too, would advise bleeding in certain cases of pneumonia. I agree with Dr. Jacobi's statement that antipyretics are useful in some cases. Only recently in a case of typhoid fever in which the Brand system was employed, the patient had some intestinal hæmorrhages, and I ceased the treatment. The woman's temperature ran up to 106°; I gave her phenacetin, and the temperature came down better than it would have done with the cold baths, but her pulse became weak and went up to 140. As soon as I could safely do so I seated this woman in an empty tub and had three basinfuls of water at a temperature of 70° poured over her head and shoulders, and I can assure you that under this treatment the heart came up better than it would have done under digitalis. The pulse fell, the temperature decreased, and the woman recovered after resumption of the Brand baths. I reiterate that enthusiasm in therapeutics is unwarranted, but that the evidence in favor of the bath treatment in acute and chronic diseases is unimpeachable.

Stated Meeting, October 19th, 1893.

MYOMA UTERI AND ITS TREATMENT.

By ANDREW F. CURRIER, M.D.

He would have been considered a rash man who had prophesied a generation ago the existing status in the gynæcological world of the subject of hysterectomy for myoma. Of the pioneers in ovariotomy, the survivors can almost be numbered on the fingers of a single hand. What a sublime work was theirs, creative of the entire vast field of abdominal surgery with its inestimable blessings to humanity! And yet, of the brave men who were operating within the abdomen fifteen years ago, there was scarcely one who did not recoil from an attack upon a tumor which, when the abdomen was opened, proved to be uterine and not ovarian. Of those who had the hardihood to attack a uterine tumor and remove the uterus with the tumor, there were few who cared to have much said about their operations or the results which attended them. Keith's record long stood peerless, almost faultless, in a field in which, by his own confession, he felt very uncomfortable and much dissatisfied. Two episodes in the recent history of myoma are of importance and interest in connection with the development of the subject. One concerns the method of treatment recommended by Schröder, which rested upon the proposition that, if the uterine canal were intact, the myomata involving only the muscular structure might be removed and the organ with its functional power be preserved. This proposition was intended, of course, to apply only to mural tumors. The tumor having been removed, the cavity which resulted was to be securely closed from below upward by successive layers of sutures. In spite of

the powerful advocacy of Schröder himself, backed by such distinguished followers as Martin and Hofmeier, this method of treating uterine myomata has never become popular. Perhaps it is well that it has been so, for the method involves much technical difficulty; it allows no avenue of escape for decomposing material, should any be present; and finally, and fatally, a uterus which is myomatous is not always cured by the removal of its tumors which are most apparent. The conditions may be repeated, and the patient is subjected to additional risk with the bare possibility of impregnation and the well-known fact that a completed pregnancy in uteri thus diseased is unusual. Therefore, unless the prospect of complete cure of the myomatous disease were offered by Schröder's operation, it would not be, in my judgment, an operation to be recommended.

The other episode—if the term be allowed—concerns the treatment of myoma uteri with galvanism, and is associated with the names of Kimball, Cutter, Freeman, and Apostoli.

Apostoli has done more than any one else to bring this method into prominence, and has introduced systematic conditions and regulations from which, at one time, a great deal was confidently hoped. I have studied this method of treatment carefully and without bias—unless it be a bias in its favor—and while I am satisfied that it is efficient in relieving the most urgent symptoms connected with myomata uteri, and for that reason not to be turned aside contemptuously, as is the habit in some quarters, I have ceased to believe that it is radically curative in its effects, except in a few rare and not always easily explained cases, and in some others in which extensive processes of disintegration have been provoked which involve great risk to the health and life of the individual. To attempt to accomplish a cure by means so uncertain is not to be commended *as a rule*. The surgical method may involve great risks, but a definite result will be reached by it, in one way or another, in a reasonably brief period of time,

which cannot be said of the method with galvanism. The most that the most ardent advocates of the electric method claim is "symptomatic cure." But if the symptoms are merely in abeyance, and the conditions for their reawakening still exist in the form of an abdominal tumor, who shall say that the symptoms may not recur, perhaps in an exaggerated degree, and find the patient less well prepared to endure them than before, or the prolonged treatment which is necessary for their relief? I am personally familiar with a number of cases in which the galvanic treatment was conscientiously carried out with such a history.

The treatment of myomata with ergot or other substances which act upon involuntary muscular fibre and impair nutrition, is another method which may be efficient in certain cases, but if these substances are used in quantities sufficient to produce their physiological effect they may do injury in addition to their beneficial effect. I have seen gangrene with septicæmia following such treatment, which was not appreciated until the tumor was removed from the body.

We now approach the important practical question: Are the symptoms which attend uterine myoma of sufficient gravity to warrant the exposure of the patient to the risks of a severe and possibly fatal operation, together with the mutilation attending the removal of an important organ? This is a large question, and it may not be possible to answer it as comprehensively as could be desired within the limits of this paper.

It was said by Keith a few years ago that he had never known of a woman dying directly from fibroid tumor of the uterus. Perhaps he may not have desired to be understood too literally in this remark, or he may not have considered for the moment the toughness and hardiness of Scotch women as compared with those of almost any other nationality. But be that as it may, there are few physicians with any considerable experience who have not seen women whose lives were rendered most pitiable and

fruitless by the constant loss of blood, the pain, and the disturbance of function attending uterine tumor, and who died prematurely with these symptoms unrelieved.

The symptoms of this disease depend not alone upon the conditions immediately associated with the tumor, but upon the sensitiveness of the individual as well, and this point has often been overlooked in the study of the subject. It is quite true that a woman of insensitive nature, especially if her life be passed in out-of-door employment, may be unconscious of the presence of such a tumor, and may be able to adjust herself to the change in surroundings as the tumor increases in size. Many negro women, many German women, are thus insensitive, but I do not remember to have ever seen a French or Irish woman who could bear the presence of such a tumor with unconcern. Of those whose lives are passed principally in-doors, and who take only such exercise as their household duties demand, there are very many to whom such a tumor is a continual source of trouble and unhappiness. The symptoms which are to be considered as legitimate attendants of myoma are hæmorrhage, pain, and disturbance of function. Consideration must also be given to those cases in which these symptoms are absent.

It is not usually the very large myomata which give rise to excessive hæmorrhage. That symptom is most frequently associated with myomatous degeneration of the entire uterus, the organ being more or less symmetrically enlarged to the size of the second or third month of pregnancy, with intramural growths, and with those cases in which the tumor is implanted within the immediate neighborhood and influence of the uterine arteries. The stimulus of the monthly pelvic congestion, and the excessive nutrition with consequent active tissue changes of the endometrial mucous membrane, are probably the chief inciting causes to hæmorrhage; and whether this consist in a continuous leakage with an occasional gush, or a sudden flooding at the menstrual epoch, the effect is

the same in the production of extreme anæmia and unfitness for the duties of life.

The pain which accompanies myomata of the uterus is often intense, especially at the menstrual epoch when they participate in the universal pelvic congestion. It is not size alone which regulates the painfulness of such tumors, for the small pedunculated or sessile tumors often cause pain which is almost unendurable. If the tumor is large enough to encroach upon surrounding structures the pressure symptoms may be distressing, but it sometimes happens that tumors which are quite large give rise to very little complaint or annoyance. The effect of constant pain, imperfectly relieved by opiates, in making the burden of life too heavy to be borne with any degree of satisfaction, is the history of many a woman with this disease. The disturbance of function which may be present varies within wide limits. Constipation is a very common and often a distressing symptom, especially if inflammatory phenomena have attended the development of the tumor and caused adherence of the rectum to contiguous structures. The functions of digestion and assimilation are also frequently disturbed either by the pressure of the tumor or by the adhesions which have accompanied the inflammation it has provoked. The effect of the tumor upon the nervous system, both on account of the pain which it causes and the mental distress and anxiety as to the possible result, is often very pronounced.

In a minority of cases none of these bad symptoms are present. The patients are sometimes entirely unconscious that anything like a tumor exists until the announcement is made by the physician who discovers it, or there may be merely the annoyance attending the increase in the size of the tumor, the sense of weight and pressure, and the mortification of the changing contour of the abdomen when the tumor is very large. Again, as in growths in other parts of the body, and especially in those in which the enlargement proceeds very slowly and the structure is very dense, there may be spontaneous regressive meta-

morphosis, atrophy, and diminution in size, or possibly the abundant deposit of lime salts. It is quite possible that a tumor of the uterus may develop, degenerate, and disappear without the attention of its possessor being called to the fact of its existence, but such cases are quite exceptional.

An interesting point which has as yet received very little study concerns the rate of development of myomata. Kleinwächter,[1] who has recently given some attention to the subject, states in his article that Gusserow and Schorler are the only ones besides himself who have published the results of their work. His study includes the analysis of forty cases in which he was able to observe their progress and symptoms. He was able in some cases to detect evidences of a tumor within three months after it had begun to grow. He observed that the rate of growth in most cases was slow—slower in the fibroids and fibromyomata than in the myomata. Temporary enlargement was often noticed during menstruation and pregnancy, and temporary diminution in size during and after exhausting disease. In only three cases did he observe permanent diminution in the size of the tumor and more or less complete disappearance. In a large number of cases not only did the tumor fail to degenerate and atrophy after the occurrence of the menopause, but it even continued to grow; and this observation of Kleinwächter's, so contrary to the teaching of the past on this subject, is not an isolated one, for experience is accumulating which shows that such a history is not at all uncommon.

In one of Kleinwächter's cases a tumor in a woman seventy years of age, which had existed many years, underwent cancerous degeneration. Martin has recorded a similar case, and so have Paget, Simpson, Tait, Schröder, Finlay, and Byford.[2] These tumors, therefore, are not as benign in all cases as has been taught, and their life his-

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[1] Zeitschr. f. Geb. u. Gyn., xxv., 2, p. 164.

[2] See Snow, "On Cancer and the Cancer Process," p. 144.

tory may be one of continuous growth which does not cease with the menopause.

What should be the treatment of such growths? In reply I would say it may be palliative, semi-radical, or thoroughly radical. No cases which give rise to symptoms should go untreated. Or perhaps the broader rule is warrantable: no cases of *tumor uteri* should go untreated, for the absence of symptoms referable to the tumor does not signify that symptoms will not come, perhaps in great severity. The palliative treatment is chiefly appropriate for those who refuse to entertain the proposition of surgical measures of relief, and also as a means of preparation for operative procedures. Great relief to the hæmorrhage, pain, and discomfort is often obtained by such treatment, but the fact and the consciousness of the presence of the tumor in the body will serve as a constant menace to the patient, horrible as a nightmare, and its growth and enlargement will usually continue, even though there may be interruptions to its activity.

First among the palliative measures of treatment is galvanism. I will not enter into the details respecting its use, as there is an abundant literature upon the subject. I have used it frequently, but never with the method of puncture of the tumor, which I think inadvisable and unsafe. I have almost always found that it would relieve pain and hæmorrhage, but never that it caused any reduction in the size of the tumor. In some cases the symptoms recurred and the tumor was removed; in others the ultimate result was not known, as the patients disappeared from my observation. That prompt relief to urgent symptoms so often accompanies its use is good reason why such thoughtful men as Sir Spencer Wells and the Keiths should be favorably impressed with the method, and it is to be regretted that its results are not more uniform and permanent. That much harm may arise from its use and militate against successful surgical treatment is a fact which does not need confirmation from those who are hostile to it; unprejudiced observers, and others who are

its advocates, can testify to the correctness of this statement. It is not fair or just to a woman with a tumor to continue electrical treatment year after year without any improvement in her condition, if any radical treatment is available which does not expose her to dangers of too grave a character. The time has come when we can speak with considerable definiteness concerning the result of operations upon the majority of uterine tumors which are brought to our notice. Palliative treatment with ergot, hydrastis, and various other astringent drugs has already been commented upon and their advantages and disadvantages noted. Treatment by dieting does not seem to me altogether a rational one, for it would be difficult to adopt any method of this character which would impair the nutrition of the tumor without impairing the nutrition of the entire body at the same time. Kleinwächter observed that after severe and wasting illnesses the dimensions of uterine myomata were diminished, but with returning health increase in the dimensions was also noticed. Curettage of the endometrium is frequently performed as a palliative measure, and it may check excessive flowing and secretion for a time, but as the endometrium is renewed and the growth of the tumor continues the bad symptoms will return. Consequently all these measures must be regarded as only temporizing or palliative ones. If a cure occasionally takes place it must be regarded as exceptional, and it must not be forgotten that much valuable time and vitality are often lost by such experiments. This fact is becoming more and more appreciated with increased knowledge and experience.

Semi-radical or tentative measures include those operative procedures in which an abdominal section is performed, the object being to cut off the sources of nutrition of the uterus and induce the climacteric. The operation necessitates the removal of the ovaries and tubes, or the ligation of the uterine and ovarian arteries, or a combination of the two procedures. The disadvantages of the operation are that the hæmorrhage for which

it was performed is not always checked and the growth of the tumor is not always stopped, even when all the arteries of direct supply have been tied. The collateral circulation may be so extensive, and the supply from the vessels in adhesions which may have formed so abundant, that the nutrition of both uterus and tumor may not be greatly impaired.

The cases in which this operation is indicated and is likely to be successful are: (1) those in which there is obstinate dysmenorrhea, due to the presence of one or more subperitoneal tumors not clearly pediculated and not exceeding two or three inches in diameter; (2) those in which there is slight myomatous enlargement of the entire uterus, with pain and profuse hæmorrhage, the uterus not exceeding four or five inches in length; (3) those in which the tumor is so massive and its attachments so firm that removal would be extra-hazardous; (4) those in which the vitality of the patient is at such a low ebb that the shock of complete extirpation could not be borne.

Operations in classes 3 and 4 of this category may be regarded as preliminary to more radical operations if the conditions ever become sufficiently favorable. In classes 1 and 2 the experience of operators in general is highly satisfactory. With the exception of prolonged symptoms peculiar to the menopause, the occasional development of uncomfortable obesity, and the regret of hopeless sterility, I have observed only the happiest results from such operations. The uterus and tumors undergo atrophy in the course of a year, and even the external genitals experience the same retrogressive changes as those which occur when the menopause comes naturally. One's opinions in matters of this character are apt to be formed by personal experience, and the fact that uniformly good results have not been obtained by all operators would not prejudice me against the correctness of the indications for operation as here delineated.

Radical measures of treatment consist either in the removal of the myoma or the removal of the tumor with

the uterus and appendages. There are cases in which the former may be done with entire satisfaction and with a minimum of danger from the operation, excluding almost all cases which have heretofore been treated by Schröder's method of enucleation. The objections to this operation, as already stated, are its greater risks, greater technical difficulties, and the fact that other tumors may subsequently develop. Those which can be easily removed are the subperitoneal ones of small size, not usually exceeding three or four inches in diameter, with a slender pedicle which can be removed close to the uterus without impairing the structure of the organ, and without leaving an extensive area of tissue denuded of its natural covering and exposed to the dangerous complications of sepsis. If the edges of the divided peritoneum are closed over the uterine wound, the vessels of the tumor having been securely ligated, no bad results in that direction should be experienced.

Excluding the forms of myoma which have been described, the treatment for the other forms should consist in the removal of the uterus, with the tumor, through an abdominal incision. This category will include the greater number of cases of myomata, fibromyomata, and fibrocystic tumors of the uterus. It will include those mural growths which are not readily enucleable through the dilated os uteri; a certain number of the submucous polypi which are imperfectly pediculated, and especially those in which a gangrenous process exists; myomatous uteri exceeding four or five inches in length, in which growth is observed to be progressive and pain and hæmorrhage very troublesome; and the subperitoneal growths, large and small, not operable by extirpation of the tumor alone, as I have attempted to describe in the foregoing. The removal of the myomatous uterus has furnished one of the most important and difficult problems in the entire range of abdominal surgery. It was not the demonstration that the uterus was an organ entirely unessential to good health and longevity which required elucidation,

and which might have been inferred from the changes which take place in the organ after the menopause, but the way to get it out of the body without such an exhibition of violence as would result in death from shock, from uncontrollable hæmorrhage, or from subsequent septic absorption.

The treatment of the pedicle after the amputation of the uterus and tumor has long been the most intricate part of the subject, baffling all attempts at successful solution on the part of many of the ablest gynæcologists who have ever lived. It seemed logical that the intraperitoneal treatment of the pedicle, which had been so unqualifiedly successful in ovariotomy, should be equally so in hysterectomy; but attempts in that direction were repeatedly unsuccessful or lacked the ideal success which was desired, the condition which continually eluded satisfactory treatment being the inability to keep the stump and its surroundings aseptic. The extra-abdominal treatment of the pedicle practised by Keith, Koeberlé, Péan, Bantock, and many others had the advantage of keeping the dangerous elements within reach, usually enabled one to avert sepsis and hæmorrhage, and in most cases offered no serious technical difficulties. But it is a clumsy procedure at best; causes an unsightly depression of the abdomen; involves constant and often painful traction on the abdominal tissues, with possible dangerous hernia should the tissues yield; is a constant menace to the freedom of motion of the intestines; and is only the exchange of a great evil for a lesser one. Far more desirable and far more satisfactory is the operation which was foreshadowed many years ago by Trask in his classical work on rupture of the uterus, in which he advised the amputation of the organ at the level of the os internum if extensive rupture occurred during parturition, the stump to be covered with flaps of peritoneum carefully stitched together. This operation has been elaborated and recommended for the treatment of myoma uteri by Chrobak in Germany, Dudley, Goffe, and Baer in this country.

The operation is in many respects an excellent one, the chief objection that could be urged against it being that it is incomplete. The vaginal portion of the cervix, which is retained to give firmness to the pelvic roof, does not seem to be required for that purpose, as can be proved in hundreds of cases in which vaginal hysterectomy for cancer has been performed; moreover, its nutrition being cut off by the ligation of the uterine arteries, it may atrophy, or even come away entirely, as in a number of reported cases. Another objection to this method consists in the possibility that decomposing material may be retained in the fossa between the peritoneum and the vagina, and give rise to septic phenomena.[1]

There remains to be described the method of complete removal of the uterus, which is practically identical with the method described by Freund in 1879 for the removal of the uterus for cancer. Freund's operation was abandoned because of its great technical difficulty, but with the improvements which have been made it would not be surprising if it again came into prominence, and even supplanted vaginal hysterectomy, with its difficulties and uncertainties, in the treatment of cancer. The improvement which has made Freund's operation not only feasible, but simple and rational, whether for cancer or myoma, is the Trendelenburg position. By its aid the structures which are to be removed can not only be felt but seen, and thus can be removed with greater safety and celerity than by any other method. The work of Eastman, Gordon, Polk, Krug, Edebohls, and others in bringing this operation before the profession, and detailing histories of cases which have been almost uniformly successful, is well known. The technique of the operation is usually simple, especially with tumors of moderate size, for which the operation is especially adapted. (I have had no experience with the operation in mas-

[1] Dr. Tucker reported a case at the New York Obstetrical Society, May 2d, 1893, in which half a pint of pus accumulated in the supravaginal fossa, and was evacuated ten days after the operation for removal of the uterus.

sive tumors weighing twenty pounds and upward, and do not know of any one who has. In such cases the technique will be more difficult than with smaller tumors.) It consists in clamping each broad ligament, as near the uterus as possible, with long and strong forceps, the appendages being excluded from their grip; releasing the tumor from adhesions; dissecting away the peritoneum from its anterior and posterior surfaces, beginning at the level of the os internum and dissecting downward until the space between vagina and peritoneal cavity is reached; ligating the upper portion of the broad ligament on one side so as to include the ovarian artery; cutting between this ligature and the clamp which is next to the uterus; ligating the uterine artery either separately or with the remaining portion of the broad ligament; cutting again between this ligature and the clamp forceps; repeating this process with the other broad ligament; separating the uterus from the vaginal attachment and removing it out of the body; ligating any bleeding vessels which may be in the cut edge of the vagina; passing the ends of the broad-ligament ligatures into the vagina as drains; passing a strip of iodoform gauze through the vaginal opening and disposing it in the supravaginal space so that there can be no pouching or pocketing of secretions; bringing together with continuous suture the flaps of peritoneum, and then closing the abdominal incision. These steps may be modified in various ways, but this is the plan which I have followed, and is essentially the one which is adopted by New York gynæcologists who practise this method of hysterectomy. The degree of shock which accompanies this operation is much less, in all cases with which I am familiar, than might be expected. This is due in part to the Trendelenburg position, which tends to avert cerebral anæmia and its consequences, and in part to the diminution in the time required for the performance of the operation compared with its prototype, the original Freund. It suggests a comparison which was made by Schröder between women

who had been subjected to Freund's operation and those who had undergone vaginal hysterectomy, the former suffering from profound shock from which in many cases they did not rally, the latter appearing like those who had experienced a moderately severe labor with rather more than the usual loss of blood. If the tumor were very large the element of shock will necessarily be considerable, whatever method be adopted in removing it, whether it be removed rapidly or slowly. The disastrous effects in such cases are not due merely to the sudden diminution of pressure by the removal of a large mass of material; in fact, in most cases no serious result need be feared from such a cause, especially if the place of the tumor be quickly filled by a sufficient quantity of dilute solution of sodium chloride at a proper temperature. The bad effects are due to the staggering blow which is inflicted upon the heart, brain, and lungs in the removal of the great volume of blood and other fluids which the tumor contains. I very much question the utility of removing enormous myomata and sarcomata from weak, enfeebled, and practically moribund women, their stock of vitality being too small for reaction. Little is said about such cases by those who are in the habit of publishing tables of their statistics. The conditions for drainage by this method are most favorable. With the broad ligaments and their ligatures turned into the vagina, and with a sufficient quantity of sterilized gauze in the vagina and supravaginal space to carry off secretions, there should be no septic complications; it is essentially an open method of treatment. With the peritoneal wound closed intestinal adhesions and obstruction are very unusual. The gauze may be gradually withdrawn, a few inches being pulled down and cut off each day until all has been removed. There is little more to be said concerning the treatment of such cases during the period of convalescence. The bowels must be kept freely open from the beginning, a bitter tonic must be given if the return to health is slow, and I prefer to use antiseptic vaginal douches daily after the gauze has

been removed. The ligatures may not come away for weeks, but that is a matter of no consequence. I have never seen a case in which the pelvic roof did not become as firm as could be desired, or the vagina present any unusual conformation which would interfere in any way with its functional purposes.[1] It may very properly be asked in these days, when many hysterectomies are being performed, Is the justification for the removal of so important an organ as the uterus, as an every-day operation, sufficient? I reply, in most cases the justification is ample. The organ is not essential to life, and, in the greater number of cases in which its removal is determined upon, is entirely inadequate to the function of gestation. If it can be removed with comparatively little danger to life, and its presence is a constant menace to health and comfort, what is to be gained by its retention? The question of mutilation has not the serious significance which it had a few years ago. If the uterus is removed the appendages should be removed also, which means the rapid induction of the menopause, relief to constant suffering, and the ability to assume anew a proper share in the duties of life without the sense of weariness and distress which the presence of the tumor made inevitable. Unfavorable mental influence as a sequel to this operation I have never seen, and can conceive of no reason why the mind should be influenced unfavorably by it any more than by the natural occurrence of the menopause. Of course in exceptional cases mental injury is possible. The influence of the operation upon the sexual appetite is likely to be governed by the temperament and previous history of the individual, just as is the case if the ovaries and tubes alone have been removed. Sexual desire does not reside in the genital organs; it is often intense when they are inactive, it may be intense if they are absent. It would seem accordant with physiological law, however, that the removal of organs associated with certain functions should

[1] Dr. Polk informs me that he has removed the uterus by this method forty-two times, with but two deaths, at the date of reading this paper.

gradually be followed by the abolition of those functions and the phenomena associated with them, and it would be unreasonable to expect that the genital apparatus would prove an exception. To those with whom sexual desire has long been absent, or has been overshadowed by the excess of suffering which has accompanied the development of the tumor, it is improbable that it will be restored by any surgical measures of whatever character. In the near future I believe we shall more frequently resort to radical measures in the treatment of myoma than is now deemed advisable, and, as in the case of ovarian tumors, we will not wait until the patient has been reduced to extremities by pain and hæmorrhage or by such extensive growth that an operation will offer unusual difficulties.

DISCUSSION.

DR. W. M. POLK.—I first desire to express my appreciation of the prophecy made by Dr. Currier at the close of his essay. It is quite in keeping with prophecies which have been made and verified with regard to other operations in the pelvis and elsewhere, so that, arguing from the past, we have every ground to believe that it will find its fulfilment. When the results which have been obtained in this country within the past few years become more widely known, it is my belief that the prophecy will receive a more rapid fulfilment than perhaps the doctor's remarks would lead us to anticipate. The mortality which faces the surgeon in the performance of hysterectomy for total extirpation of the uterus is practically no greater than that which faces him in the performance of ovariotomy. This statement may seem to be somewhat exaggerated, but if any one will take the trouble to refer to statistics which have been presented by the gentlemen who are engaged in this work, I think he will find, taking all of the cases of the various operators together, that the remark I made with respect to the mortality of the procedure will hold good. If this be the case, sir, surely the time has gone by when the operation can be condemned on the score of mortality. That issue being a thing of the past, we are now brought face to face with the question of prognosis—that is, the condition of the patient providing she recovers from the operation.

What will be that woman's state of health? I think Dr. Currier's conclusions on this point have been reached most fairly. Certainly no such reason as the possible annihilation of the sex should be allowed to influence us in a matter of this sort. Over and above this question rises the one as to the woman's utility after the operation: whether she will be able to carry on her work—her social and domestic duties, etc.—after having this important organ removed. The more I see of such cases the more fully I am convinced that the condition of these women after the operation closely approximates their previous state of good health—certainly as good health as their heredity entitles them to. They are able to perform their usual duties, and display a degree of vigor which is often surprising even to themselves.

Now we come, sir, to certain questions which the doctor has raised in reference to the state of health presented in these cases which would justify or forbid an operation; and subsequently he presents other questions bearing on the operation, which, with your permission, I will undertake to comment on in detail. First, with reference to the condition of the patient which either favors or forbids an operation. It seems to me that we would make a mistake if we endeavored to lay down a set rule to govern us on this point. In other words, we should be governed in these cases by exactly the same conditions which influence us in proposing any other grave surgical procedure; and if the patient's state of health is such as to make it probable that the measure we must resort to for her relief will result in her death, we had better not undertake the operation, unless she insists upon its performance as a last resort to relieve her from her sufferings.

The next question, sir, is one concerning the importance of the organ in itself. This point has been so thoroughly covered by Dr. Currier that it would seem to be unnecessary for me to say anything further about it. I will state, however, that there seems to be a growing impression that when the ovaries are removed the uterus has become a valueless organ, and perhaps even a dangerous one. When it is the seat of fibroid disease premature retrogressive changes may occur in its tissues, and it therefore contains the possibility of passing into a structure which in itself is threatening to the patient's life—namely, one of the carcinomatous forms of degeneration. The question, therefore, of leaving the organ in place as a possible

source of comfort to the patient should not enter into consideration at all.

There is another point I wish to speak about, and that is regarding the operation. It seems to me that the time has passed for discussing the merits of any other operation than that of total extirpation. In the formative stage of hysterectomy palliative or incomplete measures were resorted to, but we now know that we can take the organ out in its entirety far better than in part. This does not refer to operations for the removal of small single or even double fibroid tumors; it refers simply to the removal of the organ as a whole, and to the question as to whether it is better to leave the pedicle or take it out entirely. The operation of leaving the stump, as is done by Dr. Goffe, who has done so much to perfect it in this country, is an excellent one. Still, I consider the pedicle treatment as a thing of the past, and maintain that the organ can be taken out in its entirety and the pelvic floor left in as good condition as it was before. I think Dr. Currier will find, in operating on certain of these cases, that it is very much better to close the vagina entirely by turning the cut edges in and then stitching the peritoneum together from side to side over the closed vagina, making in this way an absolutely closed wound from below, which, if properly done, will never become infected, and which will give you a better result than you can possibly attain by the method he suggests. So far as the convalescence is concerned, the patient should be allowed to get out of bed anywhere from twelve to fourteen days.

The great size of a fibroid tumor is no argument against its removal. Tumors weighing twenty to forty pounds and over can usually be removed without much difficulty. The great bugbear to many operators in the removal of these tumors is when they meet with growths whose offshoots extend beneath the peritoneum and the folds of the broad ligament. Sometimes these offshoots will push themselves down behind, lifting up the peritoneum from Douglas' cul-de-sac or the lateral fascia of Douglas. There is one universal rule which covers tumors of this character, and which will enable every one to meet the demands of the case, and that is enucleation. These growths can in every instance be enucleated with the greatest ease, leaving the peritoneal covering behind; the bleeding resulting from this is so insignificant that one can easily afford to take the risk. Of course the ideal

method which the doctor has described for total extirpation, by tying the broad ligament, cannot be carried out in these cases. We must merely bear the fact in mind that there are four sources of blood supply to these organs—namely, the two ovarian arteries and the two uterine arteries, and these vessels, when the patient is in the Trendelenburg posture, can invariably be found. When these vessels are ligated the remaining hæmorrhage becomes a matter of small moment. Such as does exist comes from the vessels of the peritoneum, usually from the posterior wall of the pelvis, and they can be caught in their length and quickly tied with catgut, so that little or no blood is lost by the patient. Of course these cases are much more serious than where the tumors are symmetrical, and to remove them demands special care and greater rapidity of action.

DR. G. M. EDEBOHLS.—The subject of myoma of the uterus and its treatment is such an extensive one, and has already been so exhaustively entered into by the reader of the paper and the first gentleman who discussed it, that I think any further review of the subject in detail would prove rather monotonous to my audience. Perhaps you would be more interested in taking, as it were, a vote on the question—that is, to have each speaker confine himself to stating the conclusions he has reached; and this I shall attempt to do.

I believe the majority of cases of fibroma and myoma of the uterus, as they exist the world over, do not call for any treatment whatsoever. They produce no symptoms and therefore do not call for any treatment. That does not imply that any given case which at the present time produces no symptoms and calls for no treatment will not in the future produce symptoms and call for treatment, but there is time enough to apply treatment when the symptoms appear.

As regards the danger that is attached to the operation for the removal of a myoma of the uterus, I do not think it is any greater than that which attends the removal of an ovarian cystoma; when these cases are uncomplicated I think the chances of these patients are about the same. When adhesions or other complications exist they modify the chances equally in both conditions. Coming down to the question of treatment, and keeping up the comparison between the above two conditions, we would no more think of treating a uterine fibroid by medicinal means

than we would an ovarian cyst. Surgery is our only resource. The application of electricity for the removal of fibroids, as it is understood to-day and taught and practised by Apostoli, requires as much precaution and attention to details as does the performance of a surgical operation. I have treated fibroids by electricity, and even lost one patient by that method of treatment. At the present time, when a fibroid demands removal, I unreservedly advise the patient to have the operation done by the knife. If asked by the patient whether there is no alternative, I tell her there is, namely, electricity, but I do not advise her to try it, and if she insist upon giving it a trial I refer her to some one else.

Dr. Polk has gone so thoroughly into the modern surgical treatment of fibroids—that is, total extirpation of the uterus—that I shall not detain you by entering into that subject. The rate of mortality attending this procedure is no greater than that attending the operation for the removal of an ovarian cyst, providing both operations are skilfully performed.

DR. H. J. BOLDT.—I do not see that there is very much left for me to say on this subject. Perhaps it would be well to say a word regarding one point referred to by Dr. Currier—namely, the idea which exists that these tumors never develop prior to puberty. We know that there is one case positively on record, reported by Dr. Sutton, in which the patient, the possessor of quite a large fibromyoma of the uterus, had never menstruated. However, these tumors usually develop subsequent to the establishment of that function. As regards their causation, I believe that point has never been positively decided on. They seem to be the result of an irritation which takes place in the muscular walls of the small vessels. This theory can only hold good in the case of very small tumors. With regard to the degeneration which the growths are likely to undergo, Dr. Currier mentioned carcinoma. I have only heard of one such case. The usual change is sarcoma. The case in which a carcinoma developed was originally an interstitial fibroid; it subsequently became submucous and the endometrium underwent carcinomatous changes.

Dr. Edebohls made some pertinent remarks regarding the use of electricity in these cases; its application should be regarded as an operative procedure, and if this is not done trouble will result. There is only one class of

tumors, I think, in which we are justified in applying electricity, and that is the interstitial. In the submucous form of tumor the treatment by means of electricity should be condemned. This makes it necessary to arrive at a correct diagnosis, if it is at all possible, before advising any particular form of treatment. Castration is of no value, but on the contrary dangerous, in the case of large, œdematous tumors; but medium-sized, hard tumors are better treated by removal of the ovaries. For very small tumors, the size of a hen's egg or a little larger, ligation of the uterine arteries per vaginam has proved beneficial in some cases. I have only been able to use that treatment in one case, and the result was good. Satisfactory results have been obtained by Gottschalk of Berlin, and Martin of Chicago, and, as the operation is practically without any danger whatever, it is to be preferred to the major operation in small-sized tumors. Dr. Polk, I think, struck the keynote when he said that the size of the tumor does not make any difference as regards the danger or difficulty of the operation. Large fibromyomata, however, that develop in the folds of the broad ligament, are often very difficult to remove. Ordinarily the large-sized interstitial or submucous tumors are decidedly easier to remove than the small tumors in doing a complete hysterectomy. With regard to the complete closure of the wound after operation, I have not left the abdominal cavity open, either above or below, for quite a long time. The pelvis should be closed completely. There is no necessity for drainage, either in vaginal or abdominal hysterectomy, unless some unusual conditions exist. That abdominal hysterectomy will entirely take the place of the vaginal operation I doubt very much. Personally I am satisfied that in my own hands I can reduce the mortality in vaginal hysterectomy below that which has been obtained in abdominal hysterectomy.

DR. J. E. JANVRIN.—This discussion has already occupied so much time, and the ground has been covered so thoroughly, that very little is left to be said. There are two or three points, however, that have occurred to me in listening to the paper and the remarks made upon it. One point is in reference to the use of galvanism and curettage in these cases, and the question has arisen as to when we are justified in employing these two procedures. My own experience with galvanism has been somewhat limited. I have used it in only two classes of cases. One

is the small fibromata or myomata occurring in the young female—that is, under the age of thirty-five or forty years—and the other is in cases in which these growths exist at the menopause. In these latter cases I have usually combined its use with curettage, on account of the hæmorrhage which existed. I have had some very good results, I am sure, by the use of galvanism in young patients. I do not think the tumors have disappeared, but the symptoms have been relieved for months or years. In cases of fibroma or myoma occurring at the menopause, especially in those where the growths are multiple and located within the uterine walls (intramural), I have found that the proper use of the curette combined with galvanism has acted admirably, and in many instances I have thus been enabled to carry those patients along until the menopause was fully established and the ordinary shrinkage began to take place. This brings us up to the point as to whether in some cases the growths do not continue to grow beyond the menopause. I know from my own experience that in some cases they do. I recall one patient whose tumor increased in size considerably after the menopause had existed five years, and then took a retrograde change. I have met with other cases, but not so marked as that. Such cases have also, I think, been reported by others.

The question whether it is best to leave the cervix, or to remove both cervix and uterus, is still an open one. Until very recently my own opinion has been that it is better to leave the cervix; not, however, to leave it as Drs. Goffe and Dudley leave it (with a ligature piercing it and constricting the two sides), but to leave it with the peritoneal surfaces brought together over it, and all the vessels of the broad ligaments properly tied off by silk ligatures before bringing the peritoneal surfaces together. I must say that the cervix left in that condition as a rule does not give rise to any trouble. In fact, I have *never* seen any trouble from it. I know such cases have been reported; Dr. Polk has seen them, and so have others. In operating I am always guided by this rule: if the cervix is in a healthy condition, and it is going to prolong the operation to remove it, I leave it; if, however, it is not in a healthy condition, or if, being healthy, it can be removed without prolonging the operation, I do not leave it.

As to the degeneration of a fibroid or myoma into a

malignant condition, I have never met with such a case. I have removed many uteri by vaginal hysterectomy for cancer, and in quite a number of instances have found fibroid development in the uterine walls, but in every such case the malignant condition was confined to the cervix or the endometrium. The fact has been well established that the two diseases do occur in the same uterus and at the same time. I believe the first specimens of this character were presented by myself at the New York Obstetrical Society about five years ago. If we have a fibrous and cancerous disease in the same uterus, and the cancerous disease, which has started in the endometrium, is progressing rapidly into the uterine wall, it is fair to presume that, if a fibrous growth has any special tendency to degenerate into malignancy, such a change will take place rapidly under these conditions; I have, however, never seen it occur. In fact, in every specimen which I have seen the two conditions have been entirely separate from each other.

DR. J. R. GOFFE.—Without entering into the details of this subject, I simply wish to say that I am a very strong advocate of operative procedures in cases of fibroma or myoma of the uterus. Dr. Polk has done me the honor of referring to my method of operation wherein the cervix is left—the supravaginal operation. Most of the speakers seem to regard total extirpation of the uterus as the only recognized method of relief by operation. I do not accept that statement. I do not accept it because statistics do not bear out the statements made regarding that method of operation. Not only in Germany but in this country just as good results have been obtained by the supravaginal method as by the total-extirpation method. Dr. Zweifel, of Leipzig, has reported fifty-two cases with only two deaths. Dr. Chrobak, of Vienna, who formerly removed the cervix, has become converted to the supravaginal method and now leaves the cervix, and has reported seventeen successful cases in succession—a better record than with the other method. In this country the operation of leaving the cervix has not been performed very extensively. I have had eight cases by my method, with one death. Dr. Baer, of Philadelphia, has also adopted the method of leaving the cervix. He has modified the method by transferring the ligature from the cervix to the uterine artery in the broad ligament, and reports thirty-nine cases with two or three deaths. The

question, therefore, is still open for discussion as to whether total extirpation is preferable to the supravaginal method. By leaving the cervix it allows us to dispose more perfectly of the traumatic tissue. I have seen Prof. Polk do a number of operations, and, while I admire the way in which he disposes of the traumatic tissue, I think it better to leave the cervix and approximate the flaps over the top of it. When the operation is extended down into the vagina its dangers are increased—first, because the operation is thereby prolonged and the shock consequently increased; second, because a certain amount of hæmorrhage always attends the dissecting-off of the anterior vaginal wall, and at the end of a serious operation this may be of vital importance to the patient; and third, because the danger of injuring the ureters is greatly increased. To skilled operators this danger is almost *nil;* but we must remember that this operation is not confined to specialists, but is being performed by men of very limited experience all over the world, and for that reason we should try to make it as simple as possible. When the cervix is left there is plenty of space in which to tie the uterine artery. That is not so when the cervix is removed. Cases in which one or both of the ureters have been tied accidentally by very careful and celebrated operators are not uncommon. For these reasons, therefore, I believe the supravaginal amputation the best procedure in the removal of fibroid tumors of the uterus.

DR. A. P. DUDLEY.—It seems to me that the members of the Academy must be weary and that they are not inclined to listen with patience to anything I can add to this discussion. Notwithstanding, I feel that it is my privilege to say a few words, if only to combat some of the statements that have been made—for I like to be on the opposite side in a discussion occasionally. We have had two hours' talk about operative procedures in cases of fibroid tumors of the uterus, and I would like to ask the question, What are the hundreds of physicians outside of our large cities going to do for patients who are suffering from such tumors and who have not yet arrived at that stage wherein they would rather risk death than live with the tumor? What are you going to do for them? They cannot always be put into the hands of an expert, and still something should be done. To be sure, I must necessarily count myself among those who advise

operation. Nevertheless I do not throw operation at them the first time I see them, nor the second time. There are palliative methods of treatment which patients will eagerly grasp at, and they should be discussed as thoroughly as the radical operation. They have been very clearly described by Dr. Currier. There are many physicians in this country who are practising palliative methods in the treatment of these cases in preference to the radical operation. When we consider the few experts who are able to remove these growths by radical measures, compared with the number of women who are suffering from fibroid tumors and who are unable to be operated on, I think it will be seen at once that the important method of treatment is one which will relieve suffering and, if possible, stay the process that eventually calls for operation. Schröder's method of treatment in these cases had its day and helped educate us up to more perfect methods. It still is of value in the treatment of fibroids in single women who have not passed the menopause and whose life may be changed by marriage. I have met such cases and have operated on them, removing the fibroid and leaving the uterus, and more than one of those women is enjoying perfect health to-day. I am not frightened by the possibility of a return of the fibroid. Curetting will ofttimes relieve the symptom of hæmorrhage. In nearly all such cases it will be found that the membrane of the uterus is softened or that the organ contains a simple polypus, and if that is removed the hæmorrhage will be lessened or stopped. There is only one difficulty in curetting, and that is the difficulty of getting at the cervix if the uterus is lifted out of the pelvis by the growth.

The treatment of fibroids by electricity has, in my opinion, simply helped to lead up to the radical operation for their removal. Still, we have many followers of Apostoli in this country (I might interject here that one of our own countrymen, Dr. Cutter, advocated electricity in these cases previous to Apostoli's time), and I have seen benefit follow its use, but in order to get this benefit you must select proper cases. Such a case is certainly one that should not be operated on. Surely there must be good in it, or else such a wise man and skilled operator as Keith would not have substituted it for the knife. I am not an advocate of it, but I think there is good in it, and I believe there are those who will continue to use it.

The question of complete or partial removal has been fully discussed, and yet I would like to criticise the methods mentioned. The statement that has been made that complete extirpation is now the only method is to my mind rather strong. I think we should be guided by circumstances in these cases. I have had as good success as most operators in the removal of fibroids, leaving the cervix, but I do not mean by this that I left half the uterus. I save the contour of the vaginal vault. The semi-radical method of operation (Hegar's) I have abandoned, for the reason that if you perform a laparotomy you can as well remove a fibroid as the ovaries. This summer I operated on a colored woman for hysterectomy; I was obliged to go through the meso-colon to get at the tumor, which weighed four and a half pounds. It had to be lifted up and enucleated from the uterine walls before I could get rid of the adhesions. I then removed the uterus, which was rather difficult as some of the uterine tissue was attached to the intestines. The patient recovered without an elevation of temperature over 100°. Such cases prove that when you can get at the uterus and the tumor the growth can be removed.

DR. A. H. GOELET.—I have been very much interested in Dr. Currier's paper and the discussion following it. In some respects I consider it a very fair presentation of the subject. Still, there are some points upon which I differ with him. I have carefully reviewed the opinions of the general profession on this question, as well as those of the specialist, and believe I am correct when I say that the majority believe that when these tumors cause no symptoms or inconvenience to the patient they demand no surgical interference. Therefore, if we have at our command a remedy which will in the majority of instances remove the symptoms, it seems to me that the real necessity for operative interference is narrowed down to a very small field. My experience with electricity in the treatment of fibroids has been somewhat extensive, and I must say that I have come to the conclusion that there are very few of these tumors that are not amenable to it. Consequently I desire to place myself on record as opposed to the surgical removal of these tumors until electricity shall have been given a fair, honest, and conscientious trial by one who understands it and its application in these conditions, and not by one who would prefer to use the knife. I do not claim that this agent

will always effect a radical cure, but that it will in the majority of instances produce a symptomatic cure. I do claim, however, that in some instances it will bring about a very decided diminution in the size of the growth. As to a return of the symptoms, which I believe Martin has asserted occurs, this has not been my experience, and I have followed up some of the cases for a long time. I do not agree with Dr. Currier that we may anticipate a return of the symptoms in an aggravated form. The question is, has such a thing occurred? If so, I am satisfied that it is due to faulty technique. In spite of assertions to the contrary, the electrical treatment does not complicate subsequent operative procedures; on the contrary, it facilitates the removal of adhesions and produces a marked improvement in the general health of the patient. Cases complicated by purulent accumulations in the appendages will not of course admit of this plan of treatment.

If my experience with electricity has been more favorable than that of some others, it is perhaps because until recently my practice has been confined to private cases and they have therefore received more careful attention. I frankly confess that I do not think a dispensary is a proper place to conduct the electrical treatment in the majority of these cases, and it is unfortunate that there is no hospital in this great city of hospitals where the more serious cases, at least, could receive careful attention, where the patient could be kept under observation, and where every emergency could be promptly met.

Just a word in regard to the character of the tumors treated by this method. I quite agree with Dr. Boldt that submucous tumors should be removed when possible, but subperitoneal tumors that are not growing rapidly or have not attained a very considerable size may be treated by electricity. The treatment produces the best results in interstitial fibroids.

I cannot conclude my remarks without expressing my satisfaction with a remedy which I have seen accomplish so much good. Even if it would do no more than build up the general health of the patient in desperate cases, so as to make an otherwise impossible operation possible, it should receive only praise from the gynæcologist. The lukewarm attitude evinced toward it by the profession, and the opposition in some quarters, may be explained upon the ground of inexperience with the proper application of the remedy and therefore ignorance of its actual value.

DR. A. M. LESSER.—I have treated a number of cases of fibroma and myoma of the uterus with the galvanic current after the method advised by Apostoli, and my results have been fairly good. In eighteen cases carefully treated and observed I found but five which did not improve with the galvanic current. Those five cases I was obliged to operate on. Of course the subserous form of fibroid cannot be operated on by means of the galvanic current, and in this form the mucous membrane of the uterus undergoes really no changes. It is usually found to be normal, and we can derive no benefit from the use of the curette or the galvanic current. In the interstitial form of growth a decided benefit can be derived from the curette, while a great deal of benefit is derived from the galvanic current. The fact that the growth will return after it is once partially removed by the galvanic current I have never seen illustrated in any case, but that it may do so is no doubt possible, just as a man may recover from an attack of pneumonia and later on have pneumonia again. If the growth does recur the patient will know to whom to apply.

Since reading an account [1] of the ligation of the uterine arteries in the treatment of these cases, and the good results derived therefrom, I have tried the operation in two instances. I found it exceedingly simple. If the results are as good as promised by good men, the operation will be of great assistance to many. It is performed as follows: Make an incision through the vagina as close to the uterus as possible until you reach the broad ligament; then ligate the uterine vessels. In three or four weeks the uterus becomes diminished to one-half its size. This procedure is so simple that it can be practised by any physician who knows how to wash his hands and to make the vagina sterile. I may say that in both the cases I operated on the women were able to walk about on the eleventh day.

DR. CURRIER (closing the discussion).—As regards the size of a uterine tumor, which Dr. Polk said furnished no objection to the operation, I desire to state that in growths of enormous size, weighing thirty or forty pounds, particularly if they are malignant, the patients, in the majority of instances, have not vitality enough to withstand the operation. I also differ with Dr. Edebohls' statement that in the majority of instances a fibroid gives

[1] Centralzeitung für Gynäkologie.

no symptoms. I think a change of opinion has occurred in this regard. I was rather surprised at Dr. Edebohls' views regarding operative procedures, on account of the well-known work he has done in this field. The statement that we should wait until symptoms appear I also decidedly differ from. I think the moment such a tumor is recognized, the moment has come to interfere. We might just as well say that until an ovarian tumor causes symptoms it should be let alone.

Another point regarding very large tumors: they are sometimes found difficult to remove because of their irregularity, to which Dr. Polk referred. A prolongation or diverticulum of the growth may extend under the broad ligament or peritoneum and give much trouble. I think, therefore, that the size of the tumor is a very important factor. Nor do I agree with the statement made by Dr. Boldt that vaginal hysterectomy is preferable to the abdominal operation, and that the former is not likely to be displaced by the latter. As regards partial hysterectomy, in which the vaginal portion of the cervix is left, I stated simply what is well known, that there are cases on record in which not only accumulations have occurred in the supravaginal fossa and given rise to no end of trouble, but also cases in which the cervix has come out entirely, its nutrition being cut off. As regards the danger of injuring the ureters, I think this is obviated by the operation devised by Dr. Stimson, in which the broad ligament is split and the ureter exposed; this gives us an ideal condition; the vessels are tied in their continuity and the bottom of the pelvis is closed. The objection I have to make against the operation of Schröder, which was advocated by Dr. Dudley, is that its technical difficulties are too great for most operators. These operations are now being done by men who make no pretences to being specialists.

As regards the electrical treatment of myoma and fibroma of the uterus, I agree with much that has been said. In reply to Dr. Goelet's question, I can say from personal experience that I have seen the symptoms return in an aggravated form after the employment of electricity.

The treatment of myoma uteri is agitating the mind of the gynæcologist at the present time perhaps more than any other subject, and it seems to me that it is a subject that the general profession ought to be interested in.

Stated Meeting, November 2d, 1893.

THE IMPORTANCE OF EARLY ATTENTION TO THE DISABILITY CAUSED BY INFANTILE PARALYSIS.

By A. B. JUDSON, M.D.

I PROPOSE to consider briefly the question whether a certain class of patients should not be committed more entirely and more early than they are to the care of the orthopædic surgeon—a consideration as interesting to the family physician as to the orthopædist, as they meet in a friendly alliance to secure the greatest benefit for their common patient.

I refer to patients disabled by infantile paralysis. The child has passed through the stage of onset; ergot, electricity, and massage have produced their legitimate effect; and we will say that the eighteen months which are believed to be the limit of spontaneous recovery from the paralysis are passed. The friends and the patient, with many grievous misgivings, have become reconciled, or at least accustomed, to disability and deformity which now seem to change for neither the better nor the worse. What can now be done? The question whether such a patient may not yet receive benefit from the advance of scientific knowledge, or from the daily increasing facilities for the application of knowledge, will surely spring up in the parental heart.

Now, it is curious to note that the deformity in these cases is often found, upon analysis, to be a disability more than a deformity. Take a case, for example, in which the knee cannot be completely extended. When the patient is sitting there is no deformity, but when he stands the appa-

rent deformity is due to a disability—an inability to extend the knee. How easy it would have been to prevent contraction of the hamstrings by providing for their repeated elongation by complete extension of the knee—easy, comparatively, for one who has given himself to such details and is habitually mindful of their importance and free from the manifold cares which beset the average practitioner. And it should be borne in mind that cases sometimes occur in which shortening of the tendons begins, in a manner not well understood, at a very early stage, before simple desuetude can be fairly accused of being the author of the mischief. The prevention of muscular and tendinous shortening, then, should receive attention on the part of the early observer of the case. It is not an obscure and difficult point, but one which has perhaps escaped the consideration to which it is entitled.

To recur to the disabled knee—and this part of the anatomy is used simply as a convenient example to illustrate points in pathology and treatment applicable to all the joints—if the knee is kept extended at those times when walking is attempted, not only are the muscles and tendons kept in normal elongation, but the general welfare of the limb is assured. Neglected patients may be seen in the streets walking laboriously with extension of the knee produced by the hand pressed firmly at every step on the lower part of the thigh when the weight of the body is on that limb. It is doubtful whether this, in any case, prevents the final resort to a crutch, by the use of which the limb is made to dangle, being carried about as a worse than useless burden, twining limp around the crutch, subject to the painful affections which attack the lower extremities in cold weather in the absence of healthy circulation, and more and more impeding locomotion until, as has happened many times, the adult patient seeks relief and improved locomotor ability in amputation and an artificial limb. If the knee is stiffened mechanically the pressure of the weight of the body in standing, and the repeated concussion of the limb as the foot strikes the

ground in walking and running, will improve the tardy circulation; but beyond this, and better than this, will be the development of unused muscular fibres and special groups of muscles by whose action important motions will be acquired which would have been impossible if the limb had remained in suspension.

Now, in order to keep the knee firmly extended under the weight of the body in standing and walking, and to give use and development, as far as may be, to the fragmentary muscular system of the limb, apparatus is required; but it will not be obtained without authoritative medical advice and prescription, and, as a rule, the family physician cannot be expected to work out the tedious processes incident to treatment of this kind. It falls to the lot of the specialist, who can well bear the inconvenience attending final results, which are more or less imperfect, after satisfying himself that he has done all that science permits to be done, and whose daily and hourly mastery of the necessary details has enabled him to reduce perplexing and complicating conditions to familiar routine. Treatment of this kind lies far outside ordinary practice in which the physician renders such signal service. He makes his diagnosis, advises, prescribes, remains on guard against complications, foresees the crisis and prepares for it, leads the way cheerfully through convalescence; and medical science, so dear to us all, finds but another illustration of its power and beneficence. In one home a wasting fever has been finally resolved and the patient rises in perfect, or even improved, health from a bed where the grim alternative has been decided in his favor. In another the children have been happily carried through the perils of infection and resume their places at the table. But in the special practice adapted to the cases under consideration there is no fear of a fatal result and no rejoicing, and the object sought is at the best a palliative. But who will say that important service has not been rendered? For it should be remembered that the conservative and plodding practitioner

along this line has for an ally one of the most potent influences in Nature in the growth of the body. The popular reliance on this force, expressed in the common question, "Will the child not outgrow the ailment?" whatever it may be, is not always misplaced. Happy the physician who, imparting his confidence to the patient's friends and relying on the exact science of the physicist, sees grace and power growing out of deformity and helplessness.

A good general rule in the treatment of deformity in a growing child is to keep the part as near as possible in the desired shape as much of the time, day and night, as is practicable, so that the increment shall be on the right side of the dividing line between the normal and abnormal. The familiar proverb says: "As the twig is bent the tree's inclined." In the troubles following infantile paralysis the principle should be extended. We should not only persist in keeping the part as near the normal shape as we can, but we should also give as wide play as the crippled condition of the limb will allow to the functions of motion and weight-bearing. From the very earliest attempt to walk or to make use of the muscles and joints which are imperilled by the cord lesion, extraneous assistance should be afforded. It should not be said, offhand, that the child is too young. The first and repeated question should be: Is the child not yet old enough, or is he not already giving sufficient evidence of a desire to make use of the questionable muscles and joints to make assistance desirable or necessary?

When efficient treatment is begun it is easily continued. Improved ability is at once appreciated by both parent and child. It is well that such is the fact, for improvement does not stop with infancy or childhood. The process is slow and prosaic, but the benefit cannot be overestimated in the opinion of the one who is in the best position to judge of its value. A slight improvement in the gait, or an ability to walk a little faster or a little further without fatigue, confers lasting happiness, and the patient and physician are thus encouraged to go on to new

achievements until, with one step after another gained, the outcome is an adult well able to follow the ordinary pursuits of life, in place of a being who had looked forward to hopeless dependence.

DISCUSSION.

DR. L. W. HUBBARD.—There is only one criticism that I can offer to Dr. Judson's paper: it appears to me he has not stated the case even strongly enough. The subject of the deformities produced by infantile paralysis is one that is very interesting to the orthopædic surgeon. There is no class of deformities, I think, which, when once established, are more persistent and more difficult to correct than those following the lesions of anterior poliomyelitis. Furthermore, these deformities, in my opinion, are entirely unnecessary, and if the general profession will awake to the importance of the early treatment of these cases, we, as orthopædists, would have very few deformities to correct, because we would be called to see the case before the appearance of the deformity. These deformities occur sometimes very early in the course of the disease. Dr. Judson stated that orthopædic treatment should be commenced after a lapse of eighteen months: I should feel inclined to set a much shorter limit, and would suggest that such treatment be undertaken just as soon as there is any tendency to deformity. I do not think that the application of properly fitting orthopædic appliances will in any way interfere with the recovery of the patient. Spontaneous recovery is so rare that it is not safe to rely upon it. I find in my experience, both in dispensary and private practice, that infantile paralysis is often wrongly diagnosed by the general practitioner, unless the paralysis is very complete. The mistake is made in the minor forms of the disease, where only a few groups of muscles are involved.

In one case of this affection coming under my observation there was complete paraplegia, and paralysis of the muscles of the back, producing a marked form of lateral curvature. The boy was unable to stand and could scarcely sit alone. By the application of well-fitting braces to the back and to the lower extremities, coupled with electricity, this boy is now able to walk a considerable distance without the use of any form of apparatus, and with the supports to the back and lower extremities

he walks very well indeed. In another case, that of a little girl with complete paralysis of both lower extremities that had existed for a number of weeks, and I think months, the application of braces gave her so much support that at the end of a few months she was able to walk about, simply supporting herself with a cane.

Bearing these and other cases in mind, I can heartily indorse what Dr. Judson said in regard to the very great importance of early treatment in paralysis following anterior poliomyelitis, and the immense benefit derived from proper mechanical apparatus applied before the deformity has occurred. If we wait until they occur they sometimes require grave surgical operations for their correction.

DR. H. W. BERG.—It should hardly be necessary to emphasize the importance of early attention to paralytic deformities and disabilities, with a view to preventing the more serious deformities due to post-paralytic contractures; and yet the fact that a specialist of the standing of the reader of the paper has thought this of sufficient moment to address us upon the subject, presupposes that these cases suffer at the hands of the laity, and particularly on the part of the general practitioner, great neglect. More especially is this true of that frightful affection which makes of sturdy children lifelong cripples—infantile spinal paralysis.

Much of this apathy on the part of our profession is due to the fact that the logical medical mind remembers that the pathological cause of the paralysis is absolutely beyond our control after the acute stage has subsided. The cells in the anterior gray horns, once destroyed, cannot be regenerated; the motor power emanating from them can never be restored. What use, then, to pay any attention to muscles whose inherent motor energy has been destroyed? He who reasons thus reasons logically, but from false premises. How faulty these premises are can be best understood by considering the method of the production of the deformities following infantile paralysis.

Deformities resulting from infantile spinal paralysis are due to one or both of two causes: first, trophic changes in the tissues of the paralyzed member; second, paralysis of muscles. The trophic changes that result in deformity after infantile spinal paralysis are of an active and a passive character. The active trophic changes are those which result in the atrophy of the muscles which have been

paralyzed as a result of the destruction or interference with the trophic centre in the cord upon which they were dependent for nutrition. It is owing to these active trophic changes that the normal muscular fibre undergoes retrograde metamorphosis and is finally replaced by fat cells. These changes are organic and cannot be avoided by any preventive treatment. Whatever of shortening and atrophy is due to this degeneration, whether it affects bones, muscles, or fibrous tissues, we cannot prevent. But while these paralyzed muscles are undergoing an organic atrophy resulting in shortening and attenuated muscles, other parts of the limb not paralyzed are undergoing a non-essential atrophy due to disuse and stoppage of growth. Now, if in any way the function of the paralyzed muscles can be replaced to a greater or less extent by orthopædic appliances, it is evident that at least the healthy muscles will, in consequence of even the limited use to which the limb is thus put, maintain their function and health, and perhaps even, to some extent, learn in time to replace in activity the muscles that have been paralyzed. Thus, for example, when the orthopædist rectifies by a high shoe the shortening which results from atrophy of a paralyzed leg, he will prevent the formation of a functional lateral curvature of the spine. Left to Nature alone, the pelvis of a patient who attempts to stand upon a shortened leg will become oblique, the normal side being upon a higher plane than the paralyzed side. Many other examples could be given in which the early rectification of shortening prevents the production of new secondary deformities and the aggravation of those already existing.

By far the greater number of deformities resulting from infantile spinal paralysis are due to the paralysis of muscles and consequent contractures of healthy muscles in the affected limb. Paralysis of muscles always produces disability, but the resulting contractures in non-paralyzed muscles produce deformity. Thus contracture of the muscles attached to the tendo Achillis following paralysis of the anterior tibial group causes a paralytic talipes equino-varus. In other words, the deformity is secondary to and increases the disability produced by the paralysis. If, then, this contracture of the tendo Achillis could have been prevented by orthopædic means, we would have prevented deformity and so much of the disability as is dependent upon the deformity.

When we understand how such secondary muscular contractures are produced, we can apprehend the orthopædic principles by which they can be prevented. It was formerly observed that the muscles of the body are grouped in two such series that one seems to antagonize the other in its action. Finding that frequently, when a muscle became paralyzed, its opponent became contracted and finally contractured, it was laid down as a rule without exception that when a muscle becomes paralyzed the part ordinarily moved by that muscle is given over to the influence of its antagonist, and this antagonist becomes contractured—as, for example, the production of the equino-varus just cited. But a little clinical knowledge of paralytic cases will show that this rule is not of invariable applicability. Take, for instance, cases in which all the muscles of the lower part of the leg are paralyzed. Here the deformity which results, if any, is talipes equino-varus—surely not owing to contracture of the antagonists of the anterior tibial group, for the posterior tibial group is also paralyzed. The cause of the equinus in this latter case is evidently a shortening of the tendo Achillis, due to the action of gravity, which causes the foot, as it hangs on the leg, to assume the position of talipes equino-varus, in which position the muscles are fixed by their future growth.

Without entering too deeply into the subject of the production of contractures, it will be easy to study it upon a recent case of facial paralysis. Immediately after the paralysis has occurred on one side of the face, when the patient first attempts facial movement on the non-paralyzed side the face is slightly drawn toward that side. I say slightly, not completely, as would be the case if the cause of the deviation was the unopposed action of the healthy muscles. What has occurred is that, as soon as the motor impulse to the healthy side of the face has ceased, the muscles relax almost to their normal condition; the residual work necessary to complete the restoration the opposing muscles perform for each other; the opponents in our case being paralyzed, this restoration is not completed and the origins and insertions of the healthy muscles are brought nearer to each other; thus the muscle is slightly shortened. This shortening becomes more marked with every motion until contracture of the healthy muscles results permanently. This, it will be found, is the most logical explanation of contractures and

the one most applicable to ordinary paralyses, although in the lower extremity the force of gravity may aid in the production of deformity, as in the instance cited. The theory of unopposed muscular tonus is certainly untenable.

It is evident, then, that orthopædic appliances which will do the residual work necessary to complete the restoration of healthy opponents to their normal position when they have relaxed after contraction can prevent contractures in such non-paralyzed muscles. Inasmuch as such contractures take time for their production, the sooner the case is treated the better. The view of the reader of the paper that such treatment is more apt to be successful in the hands of the expert is a truism. Thus the equino-varus in the case instanced above could have been prevented by replacing the power of the paralyzed muscles by the power in elastic straps applied on the Barwell principle. A similar elastic band posteriorly could replace the power of the posterior tibial group when they are paralyzed and the anterior tibial group is healthy. Not only is deformity prevented and thus the disability which depends upon the deformity, but even the disability due to the paralyzed muscles itself is to a great extent neutralized. This principle of the substitution of elastic bands for muscular power can be carried out in various portions of the body by proper orthopædic combinations of steel and elastic.

A work could be devoted to the uses of tenotomy, mechanical appliances, and massage in the prevention of deformity and the cure of disability resulting from infantile spinal paralysis and consequent deformities. Surely the earlier such cases are sent to the orthopædist the better the results we may hope to attain.

DR. H. KOPLIK.—When I was invited to discuss this paper I felt that I would have very much preferred to come and simply listen and learn. I think the orthopædist should allow the general practitioner a certain amount of discretionary power in dealing with these cases of infantile paralysis. It is sometimes difficult to decide when the case should be referred to the specialist. The doctor does not care to overload the orthopædic clinic with cases that do not present indications for immediate orthopædic treatment. These cases do not all present the same features. I agree with Dr. Judson that the sooner unfavorable cases are referred to the orthopædist the

better. On the other hand, some paraplegic cases recover partially or even entirely, and the physician wishes to keep them under observation and see what set of muscles will eventually recover. There are cases in which at the very beginning it will be apparent that certain sets of muscles will never recover. We can tell this by the electrical reactions. When we ascertain that a muscle will not recover, that it will remain permanently paralyzed, then the case is no longer one for the physician to treat, but for the orthopædist. This may be at a time when there are no signs of deformity; nevertheless the indications are to place the case immediately in the hands of the mechanical orthopædic surgeon. When I find, upon electrical examination, that a certain muscle or group of muscles gives that electrical reaction and that clinical history which point to the fact that such muscles will never recover, I send them to the orthopædist, even before deformity occurs. I do not think there is any excuse for waiting until contracture occurs. The exact period of time which should elapse before the case is referred to the specialist is variable. Sometimes it can be determined after fourteen days or a month. Sometimes it requires six months, which is probably the average. I do not think we should wait eighteen months, the period stated by Dr. Judson.

DR. E. D. FISHER.—The subject brought up by Dr. Judson has been thoroughly discussed. A question that is often asked by the parents of these children is, Will the child recover the power and use of its limbs? The interference with the growth of the bone must always be borne in mind in giving a prognosis. The injury to the spinal cord cannot, as a rule, be much affected by treatment. The only thing we can do, in my opinion, is to recommend rest and quiet. Then we come to the after-treatment, such as electricity, etc., and the question arises, how soon shall we apply it? When the child is very young—that is, when the disease occurs at the age of twelve or eighteen months—I am a little cautious about advising the application of apparatus of any kind to the limbs, unless a marked deformity exists two or three or four months after the onset of the disease; in that case, whatever the age of the child, I believe in having a proper apparatus applied. I do not quite agree with the statement made that these cases should be turned over to the orthopædist as soon as we get the reaction of degenera-

tion in the muscles—that is, as soon as we find that there has been a serious lesion in the cord. I would not even say in such a case that the power of the muscle is absolutely lost. It is possible that even with the reaction of degeneration the muscle may recover. This does not often occur, but it may. In very young children, unless marked deformity is present, I prefer to wait, simply keeping up the nutrition of the muscles. Let the child move the limb as much as possible. Just as soon as I observe any interference with the movements of the child after the inflammatory stage has passed, with or without deformity, I then advise that some apparatus be applied. It is a great error to allow these patients to become bed-ridden or chair-ridden. Therefore the orthopædist should be called in immediately if that occurs. In this way you will better preserve the symmetry of the limb, and furthermore, by taking the muscles and exercising them in any way you wish, especially by natural means, you encourage, by the reflex action of the motions of the limb, the nutrition of the cord itself, and thus you may save some of the cells which are not entirely destroyed. Therefore, to my mind, just so soon as we see the need of action or motion on the part of the patient, and just so soon as we see deformity, we should advise the application of proper mechanical appliances. Of course we understand perfectly well that it is not within the power of the physician to save a limb that has been interfered with in its growth or nutrition by a serious injury to the cord.

DR. W. R. TOWNSEND.—I wish to take exception to one statement made by Dr. Fisher. He does not believe in putting on any mechanical appliances, or in any way hindering the movements of the child, until deformity occurs, or where the child, without instruments, has a fair amount of control over the limb. It seems to me that if you wish to restore the tone and power and action of the muscle, it is necessary that that muscle should act as far as possible in its normal relations, and this it cannot do unless you keep the limb in perfect position. The child may be able to move about and possess considerable motion in the limb, but the motions are awkward and done badly, and in order to correct this the limb must be kept in its proper position.

DR. A. D. ROCKWELL.—I hardly feel competent to discuss this subject from the standpoint of the orthopædic

surgeon. Still, my experience in the treatment of infantile paralysis has not been inconsiderable, and I suppose this experience has been very similar to that of others, and that is, in order to obtain good results in these cases, persistent, well-directed effort is necessary. I was interested in the remark made by the reader of the paper to the effect that the results of the disease were probably more those of disability than of deformity. This perhaps is so, but in all prolonged cases we have wasting of the muscles and degeneration of tissue, and these factors essentially constitute deformity rather than functional disability. The cord itself suffers from degeneration. In some cases of infantile paralysis wherein the symptoms are profound, these resolve themselves and the patient approximately recovers.

Electricity has been referred to by a number of the speakers in connection with the treatment of this affection. Of course it is of value, although it has been overestimated; certainly there is no evidence that it is curative in its action, or that applications to the spine have the slightest influence in restoring the nerve element, but, if it does not do this, it does improve the nutrition of the muscles and reflexly acts in a beneficial way. The faradic current is of no value, because the muscle does not respond to it; this is well known, but perhaps it is not so well known that the galvanic current is capable of doing a great deal of harm or good, according to its method of application. I have great faith in the continuous action of the current, while the interrupted current may in some cases be absolutely harmful. I recollect one case wherein the feeble vitality remaining in the paralyzed muscle was completely extinguished by injudicious attempts at contraction with the galvanic current; while in another case wherein the continuous current was applied, which in its action upon nutrition is reconstructive rather than destructive, the slight vitality in the muscle was saved and considerably improved. Of course electricity does not do everything, nor perhaps very much, and in many cases mechanical means are required; especially is this the case where we have shortening of the muscles with resulting deformity. Where we have the tibialis anticus and the peronei muscles affected I believe the orthopædic surgeon should be called in at once.

DR. H. L. TAYLOR.—After an attack of infantile paralysis the problem usually presented is how to make the

most of the mechanical and vital possibilities of a damaged limb. There is at first a well-marked tendency to recovery in many of the paretic muscles, and this can be favored by keeping them in their natural relations by a mechanical appliance. In paralysis of the ankle flexors, for instance, the anterior tibial group should be protected from the damage of stretching by mechanically preventing the foot from dropping beyond a right angle. This can be done without impediment to flexion and without harmful constriction, and benefits the calf muscles as well as the flexors, since it prevents the former from becoming permanently contracted. In a similar manner nearly all the deformities so frequently following poliomyelitis can certainly be prevented.

Dr. Judson has said that there is no fear of a fatal result and no rejoicing in treating these cases. On the contrary, when a child is enabled to walk off at once on the application of proper apparatus, after years of utter helplessness, as I have several times witnessed, words cannot describe the keen rejoicing of the privileged spectators. By making locomotion possible in such a manner that weak parts are not only protected from injury, but stimulated to develop, and many hitherto inactive parts brought into use, the circulation and nutrition are stimulated; and it should not be forgotten that many dormant nerve centres are brought into play, and also that the interests of the child are wonderfully broadened. I have not only seen cold limbs grow warm and helpless ones active, but shyness and apathy give way to cheerful activity, after the application of braces had enabled the patient to walk. Inefficient support is often worse than none, because if the patient tries to walk when the apparatus causes pain, or fails to hold the limb with precision, he will inevitably shrink and restrain harmonious action. Many of the failures charged to mechanical treatment are due to lack of skill or lack of thoroughness. In protecting weak muscle groups, use is often to be made of the principle of "limited motion," giving the patient the stimulus and benefit of a certain amount of motion within safe limits. However, since Dr. Goldthwait, of Boston, has shown that muscular power remains good after so long as fifteen years of disuse, we need not fear to fix a joint in a correct position, if it were necessary, rather than condemn a person to helpless deformity.

There is another point that I wish to mention regard-

ing treatment, and that is the persistent use of dry heat to the limb. I know of nothing that helps the circulation so much in these limbs as the application of dry heat. I apply it by means of a small hot-box in which the limb is placed, the temperature being raised to 160° to 170° F. The limb is allowed to remain in the box for fifteen or twenty minutes once or twice daily.

Certainly the importance of early expert attention to the disabilities caused by infantile paralysis cannot well be exaggerated.

DR. A. M. PHELPS.—I regret that I was not here in time to listen to Dr. Judson's paper. The doctor kindly sent me an abstract. There is only one point in connection with it, however, that I desire to discuss. The important point is, at what stage of the disease shall the general practitioner turn these cases over to the specialist? Unless the general practitioner is well versed in mechanics, I do not think he should ever undertake to treat these cases if they are able to walk about—that is, if they are over two years of age. If such a child is properly braced there is no question that the nutrition of the limb is improved. The temperature of the limb improves and it is better in every way. Then, again, if the limb is not properly braced contractures of the muscles take place and deformity must necessarily follow, and unless one is well versed in the diagnosis of these troubles it is rather difficult to tell just when they have taken place. The orthopædic clinics are crowded with cases of neglected spinal paralysis. In many of these contracture and deformity is marked. In such cases, instead of carrying out a prolonged period of treatment by braces, etc., as is advised by some, I believe it is better to operate, dividing the tendons subcutaneously and putting the foot into the correct position, and holding it there by means of very light braces. Frequently you will find groups of muscles in the foot or leg paralyzed. It is an easy matter then to take the tendons of the toes, where these muscles are not paralyzed, and graft them into the gastrocnemius, and then a light brace will prevent deformity. Excision of knee and ankle to produce ankylosis is often advisable, as it makes a useful crutch for the patient to walk upon. There is a great difference of opinion among orthopædists about dividing tendons in these forms of paralysis. I have brought here to-night a patient whom I presented about a year ago, to illustrate the fact that

dividing tendons is preferable, in these cases, to prolonged bracing. This boy was treated by a number of eminent orthopædists, both abroad and in this country, and it was decided on consultation that he undergo a prolonged series of mechanics of different kinds for the correction of the deformity he possessed. I simply divided the tendo Achillis and cut whatever tissue was contracted; I then pulled the foot down into the straight position and braced it, and now the boy's feet, as you see, are in very good shape. He is able to raise himself upon his toes, and no tendency to relapse is observed. He has discarded his braces. Such good results are not uncommon in cases where the tendons have been divided. This boy, when I first saw him, could not get his heels to the ground and already had commencing equino-varus. After the tendons were divided and he had been under treatment for six weeks, he could walk almost as well as he can to-night.

One further point, and that is in regard to instrument-makers and bandagers. These cases of infantile paralysis too often fall into the hands of the instrument-maker, who puts on a brace and usually makes the case worse than it was before. I should be in favor of having a law passed to prevent this.

THE PRESIDENT.—I think the present law covers such cases.

DR. R. H. SAYRE.—It seems to me that many of these cases of infantile paralysis can be treated with a fair degree of success by the general practitioner. The disease is not uncommon, and often occurs in places where there are no orthopædists, and the general practitioner, if he possesses some knowledge of the subject and exercises common intelligence, can prevent the occurrence of at least one-half the cases of deformity that finally come to the specialist. A large part of the work of the orthopædist consists in undoing or rectifying deformities which should never have taken place.

There is very little to discuss in Dr. Judson's paper. We all agree with his statements, which are incontrovertible, but the general practitioner has no right to allow the deformity to become so far advanced that at last the patient, in self-defence, is obliged to seek the specialist. The general practitioner ought to familiarize himself sufficiently with the principles of mechanics involved in these cases, so that he can apply them and keep the bones

and muscles in their proper relations. In addition to the contracture of tendons, a great many, and often the worst, deformities following infantile paralysis are caused by the abnormal growth of bones. If the joints are not retained in their normal relationship to each other during the period of growth, marked deformities may arise, such as knock-knee or clubfoot, in consequence of unequal bone growth, and it is often impossible afterward to enable these patients to walk without the performance of extensive operations which could have been avoided by retaining the proper relation of the joints in the commencement. It seems to me that the general practitioner should be more alert to observe these incipient deformities and apply measures to correct them.

I was somewhat surprised by the statement made by Dr. Rockwell that electricity was of no use in developing paralyzed muscles, because my observation has been distinctly at variance with the views expressed by him. A great many of these paralyzed muscles are paralyzed only in certain portions; there are good muscle fibres remaining, and in some instances where the patient is not able to contract this muscle with sufficient power to move his extremity, it can be caused to contract sufficiently well by the galvanic or faradic current to exercise the limb. The gymnastic exercise which these healthy muscular fibres receive in this way has been, in my experience, of distinct benefit to them. I have certainly seen great benefit derived from faradism, as well as from galvanism.

Dr. Koplik's statement that after fourteen days the question can be answered as to whether a muscle can be regarded as hopelessly paralyzed or not, is one that I have not been able to verify, and I should like to hear on what grounds he bases his assertion, as I have seen muscles, that failed entirely to respond to electrical stimulation of any sort, later on recover power to a very considerable extent. Degeneration reaction simply gives us information as to the parts of the muscle that we have stimulated, but there are often fibres situated out of our reach which are not dead, and which later on surprise us by giving contractile power to the muscle.

DR. A. D. ROCKWELL.—Dr. Sayre certainly misunderstood my statement regarding the value of electricity in these cases. I stated that it was of no value when applied directly to the cord, but of decided value when ap-

plied to the muscle for the purpose of increasing muscular nutrition.

DR. KOPLIK.—My statement regarding the time when these cases should be referred to the orthopædist has not been understood. As soon as I find out that the nerve does not respond to the faradic or galvanic stimulus, and that the muscle itself does not respond to the faradic stimulus, but does respond in an increased degree to the galvanic stimulus, I take no chances on the case. I think most of us will agree that when the degeneration has gone as far as this the muscle will never recover. As to the exact time when this occurs, I stated that that was variable.

DR. A. JACOBI.—The remarks of Dr. Sayre have given me the courage to rise and say something from the standpoint of the poor general practitioner, whom Dr. Sayre tried to redeem. We have been told here to-night that cases of infantile paralysis are overlooked by the general practitioner; we were then told by another speaker that these cases are neglected by the general practitioner. First, therefore, the general practitioner overlooks the cases, and then he neglects them.

We have furthermore been told that these cases must be referred to the orthopædic surgeon very soon—within a few weeks, perhaps, or within a few months. Also, that the orthopædic surgeon alone can do the necessary things. How does he know that? Does he know how the cases are treated in general practice? He only sees the neglected cases, and I admit there may be many of them. But he does not know that many of these cases are correctly treated by the general practitioner, who has profited by the teachings of the pathologists, the surgeons, and the orthopædists of the past twenty or thirty years. Just such measures as have been recommended here to-night were recommended forty or fifty years ago, since Heine wrote his monograph in 1840 in which he advised mechanical measures, and at least thirty years ago I assisted Dr. Krackowizer in the application of such mechanical appliances in these cases.

It is true that most of our efforts will be futile in these cases; I have never in my life seen a case that entirely recovered. I believe, however, that a few stray cases have ended in complete recovery. When a neurologist sees such a case he is triumphant over it. I do not believe the orthopædists have any better success. The

course of the affection is as follows: There is a sudden paralysis of certain muscles or groups of muscles; this remains complete for a few days and then begins to improve, very rapidly for a week or two, fairly rapidly for seven or eight weeks, very slowly for a few months, and then it stops. Before the end of the first month I do not think that deformity or contracture ever occurs; there is simply paralysis. The disease affects the lower extremities in the large majority of cases—I believe, in sixty per cent. In some cases there is paraplegia, in others monoplegia; in some cases the upper extremities are affected, and in a very few cases all four extremities are involved. Deformity is rare during the first few months of the disease, and sometimes does not occur for six months or a year. The more common deformities are clubfoot and knock-knee. What can we do for these cases? We can apply massage and electricity, and I am decidedly of the opinion of Drs. Rockwell and Sayre that both faradism and galvanism often prove of much benefit. It must be applied regularly and persistently, and the general practitioner must exercise just as much patience as the orthopædist if he wishes to get good results in these cases. The hot bath, passive motions, friction—all these the general practitioner can apply. Strychnine, I believe, also does good in these cases after they have passed the first stage. The drug should be given subcutaneously, not internally, once every day or every other day. Where strychnia given by the stomach will do absolutely nothing, a small dose of the drug given subcutaneously will often excite the contractility of the muscle considerably.

Do I mean by all this that the orthopædist should not be called in? Certainly not. I am called continually to see patients whose ailment the attending physician understands perfectly well. The general practitioner is not afraid to meet the orthopædist in consultation in these cases. To say that the general practitioner is good enough to prescribe calomel or treat typhoid fever, etc., but that he is tabooed in a case of infantile paralysis, is, I think, overestimating the merits and importance and future of the specialist. Why are we not told that when the electrical current is to be applied the neurologist, or rather the electrotherapeutist, must be called in to do it?—for indeed I claim that the electrical current requires as much skill as a mechanical contrivance. I also suggest that when strychnia is to be injected the general practi-

tioner and the orthopædist call a surgeon. I am in the habit of attending as many meetings of specialists as possible, and I generally learn something there. I learn a little more about the special anatomy of a part, a little dexterity, a little that will do good to mankind in my hands. To-night, however, we have learned little from the discussion excepting that we must turn these cases over to the orthopædist. I was delighted to listen to Dr. Phelps, however, and to see the results he has obtained by means of tenotomy. Probably my next case will be operated on by Dr. Phelps. I failed to learn, either from the discussion or from the paper, what the general practitioner should do in these cases. How should he support the limb, and how is the apparatus to be applied for the purpose of supplying deficient muscular power? I do not mean any harm in these remarks, but I wish to speak my mind, and my mind is that all these cases should not be referred to the orthopædist, but rather that the general practitioner should not deprive himself of orthopædic knowledge. I do not believe that these cases require so very much special knowledge. I have ofttimes seen them successfully treated by the general practitioner. I still believe in the general practitioner, and I believe in his future, if he will persist in understanding and treating cases, and not confine himself to keeping a distributing office in the service of specialists.

DR. JUDSON (closing the discussion).—I have nothing to add to the consideration of this subject. I am gratified to know that in the hasty preparation of my paper I so nearly expressed the opinions of the members of the Academy, both specialists and general practitioners.

Stated Meeting, November 16th, 1893.

MYXŒDEMA, ACQUIRED AND CONGENITAL, AND THE USE OF THE THYROID EXTRACT.

By GEORGE W. CRARY, M.D.

But little new light has been thrown upon the morbid anatomy of this disease since the publication of two autopsies by Hun and Prudden,[1] and of the report of the London Clinical Society,[2] in 1888, and I shall use largely their conclusions here in describing the post-mortem changes found in adults.

The Thyroid Gland: This is constantly the seat of disease. Usually much diminished in size, with excessive atrophy of its parenchyma and an increased amount of connective tissue. These changes are progressive, and in some cases there is left little or no true gland substance. Hun and Prudden also found "new formation of lymphatic tissue in the thyroid."

The Skin: Separation of superficial layers of the corium. Atrophy of hair follicles (Hun and Prudden). Obliteration of lumen of sweat glands and of sebaceous glands.

The Arteries: Obliterating endarteritis with atheromatous degeneration, and in places amyloid degeneration.

The Heart: Hypertrophy of left ventricle (Hun and Prudden). Interstitial myocarditis (Clinical Society).

The Nervous System: Chronic diffuse neuritis only.[3]

The Kidneys: Chronic nephritis.

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[1] American Journal of the Medical Sciences, July and August, 1888.

[2] Report of London Clin. Soc., 1888, Committee on Myxœdema.

[3] American Journal of the Medical Sciences, July and August, 1888. Greenfield: Edinburgh Med. Journ., May, 1893, p. 1048.

Interstitial changes occur also in the liver, the submaxillary gland, and in the adrenals.

The Blood: Diminished number of red corpuscles. White, normal. Diminished percentage of hæmoglobin.

The presence of mucin in increased quantities in the skin and other tissues does not seem to be constant in myxœdema, but would seem to be a marked feature in the strumipriva of lower animals.[1]

As to the pathology in the congenital variety, I have seen no report of autopsies performed. Hofmeister[2] removed in young rabbits the thyroid gland, but left the glandules of Gley, thereby leaving enough gland tissue to maintain life, and found that the hair became coarse, the body became short and thick, the growth of the skeleton was arrested, the epithelium of the convoluted tubules of the kidneys was altered, and enlargement of the hypophysis cerebri occurred.

Myxœdema is a disease of all ages, occurring at any time from the period of intrauterine life to old age, but most commonly making its appearance, in infants, shortly after birth; in adults, in women near the time of the menopause. In the acquired form in adults the clinical pictures presented when the disease has reached full development show but few variations. In these cases the appearance is striking and characteristic. The facial expression is dull, heavy, and mask-like, and the natural lines are obliterated. The face is large, and especially broad at the lower part; the skin is dry, sometimes smooth, cold, and waxy, sometimes wrinkled and scaly, but always thickened. The eyelids are swollen, sometimes to such an extent as to nearly close the eyes, and the constant effort to elevate the upper lids produces a characteristic arch of the eyebrows. The nose is swollen and the bridge is flattened. The lips are large and thick and protrude, the lower one being pendulous, and they

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[1] Horsley: British Med. Journ., January 17th and 31st, 1885.

[2] Putnam: Trans. Assoc. Amer. Phys., 1893, viii., p. 343; American Journal of the Medical Sciences, October, 1893.

are of a bluish or violet hue. The tongue is large and thick, and can be moved about but slowly and with difficulty. The fauces are swollen and the mucous membranes pale. There are quite constantly spots of hectic over the malar bones, which are in marked contrast to the color of the rest of the face, and there are often seen upon the sides of the face patches of chloasmata. Occasionally the whole face, and also the hands, will be of a yellowish tinge, and more rarely are stained a dark mahogany, simulating Addison's disease. The hair upon the head is thin and coarse, and the scalp dry and scaly. The whole body is swollen with an œdema which does not pit upon pressure, although occasionally there will be associated with this condition, usually most marked in legs and feet, the ordinary œdema which will pit. Hun and Prudden explain the non-pitting nature of the œdema in this disease by the fact that it is largely confined to the superficial layers of the corium, where the spaces are so small and the interlacement of the fibres so fine that the fluid is not easily driven away by pressure. The skin is everywhere waxy, dry, and with more or less desquamation, sometimes in large flakes. A total absence of perspiration is the usual condition, although occasionally the body, and especially the neck and shoulders, will become temporarily covered with a clammy moisture. In my first case[1] phenacetin and hot mustard foot baths caused no sweating, and sinapisms no redness. There is little or no hair in the axillæ or upon the pubes. The nails and teeth are imperfect. The gums are spongy. The swelling is most marked in the clavicular regions, and often there exist here, and in the lower abdominal region, fatty tumors which are not usually sharply circumscribed. The whole abdomen is large and pendulous. The swelling of the skin over the breasts often entirely hides the nipples. The swelling of the hands and fingers, and the dryness and inflexibility of the skin, cause great inconvenience to

[1] Medical Record, June 17th, 1893.

the patients. There is nearly always present dysphagia, and this is often so great that even liquid food can be swallowed only slowly and with difficulty, and solid food will be regurgitated. The swelling of the nasal mucous membrane makes it difficult or impossible to breathe through the nose, and there is loud snoring during sleep, and "blowing the nose" is accomplished in an unsatisfactory manner. The breathing is usually slow, and if for any reason it is quickened, dyspnœa results. These attacks of dyspnœa are often a strongly marked feature. The speech is slow and monotonous, and is almost pathognomonic in its peculiarity.

The cutaneous sensibility to cold is great, and these patients are invariably worse during cold weather. There are usually subjective sensations, as of cold water being poured down the back or dropped upon the face or body. Deafness is usually present, and sometimes is marked. Smell and taste are sometimes impaired. There is paresis of all the muscles, and this is often so marked that the patient is scarcely able to stand without support, to lift anything with the hands, or to keep the head from sinking forward on to the chest. The movements of these patients are clumsy and the walk is waddling. Bramwell[1] aptly describes them as "hippopotamus-like." Hæmorrhages from mucous membranes are nearly always present, usually in attacks of varying severity, and at night they are prone to stain the pillow with a brownish fluid which comes from the mouth or nose. Rheumatoid pains are often complained of, especially in the wrists, ankles, and back, and there is often pain and tenderness on pressure along the sterno-mastoid muscle on one or both sides. Headaches are generally complained of; anaphrodisia is common, and intercourse is usually painful in women. The temperature is low, ranging between 95° and 97.5°. Pulse usually about 70 and small; respirations between 17 and 18; heart sounds feeble and muffled. Urine always

[1] Edinburgh Medical Journal, xxxviii., p. 985.

of low specific gravity, between 1008 and 1014; usually acid, and containing a diminished amount of urea; albumin in small amount is generally found, and, as a rule, there are hyaline and granular casts. Irritability of the bladder, with frequent micturition, exists. The bowels are costive, the appetite is poor, and no special kind of food is craved. The mental condition presented by these cases is varied. There is present a condition resembling mild dementia, with lack of interest, listlessness, mental weakness, melancholia, and hallucinations. Dr. Starr, who has fully described the mental state present in these cases, further states[1] that "it is thus evident that both the afferent channels of communication from the external world and the efferent paths of impulse to the muscles were in some way blocked by the disease, while the internal processes of mental association and activity were also hampered and deflected from their ordinary course."

These features are usually all of gradual development, the disease running a very chronic course from the appearance of the first symptom, which may be the lassitude and indisposition to exertion, the paresis, or the swelling. The onset may, however, be sudden, but more commonly is very gradual. The course of the disease is chronic, lasting from the appearance of the first symptom until the death of the patient many years afterward. There are occasional periods of amelioration of many of the symptoms, and these patients are always benefited by warm weather. Bramwell states that the disease tends to inhibit childbearing, and that pregnancy induces more rapid development of the symptoms; but the case reported by Kirk,[2] in which the disease began at twenty, and the patient, subsequently marrying, bore eight children, her condition being much improved during the periods of pregnancy, would show that this statement has exceptions.

When death occurs without complicating disease, a

[1] Trans. Amer. Phys., 1893; New York Medical Record, June 10th, 1893.

[2] Lancet, September 23d, 1893; Case iv.

fall of temperature with coma usually precedes the end. The similarity of the mode of death to that due to uræmic poisoning suggests that the degeneration of the kidneys is the principal factor in causing a fatal termination.

In infants the disease presents special features by which it has been separated from myxœdema and classed as a disease by itself—namely, cretinism. When occurring outside of those places where goitre and cretinism are endemic, or even epidemic, and probably due to local causes, the term sporadic cretinism is used. This condition was supposed to be intimately associated with myxœdema as early as 1873, when the first clinical study of the disease by Gull was published; and since the report of the London Clinical Society in 1883 this connection has been fully appreciated. Osler[1] points out the necessity of limiting the term cretinism strictly to those cases due to loss of function of thyroid gland, whether this loss be due to congenital or progressive atrophy or even coarse changes. The special features referred to above are due to the arrest or retardation of growth and development, both mental and physical, which takes place in the congenital myxœdemic from the time when the diseased condition of the thyroid has reached the degree necessary to affect the body nutrition. This may occur at varying periods of intrauterine life, so that at the time of birth there may be the supraclavicular fatty tumors, the thick redundant skin, the swelling of the hands and feet, the short extremities with the large, soft epiphyses and ossified apophyses, and the broad skull with wide-open sutures—making the diagnosis plain. These children usually die very soon after birth. In other cases the child is normal in appearance at birth, or there may exist a goitre, but at any time from then until it is five years old (Osler) the myxœdematous symptoms may begin to appear. I am inclined to believe that in most cases the evidences of sporadic cretinism begin to appear early, but that, being

[1] Transactions of the Association of American Physicians, 1893; American Journal of the Medical Sciences, November, 1893.

at first of the nature of simple arrest of development, they do not for some time cause comment. The myxœdematous symptoms develop gradually, but at an early period in the life of the child the appearance has become characteristic. During the natural period of growth the changes become exaggerated, and an adult congenital myxœdemic, between twenty and thirty years of age, "is a being degenerate both physically and intellectually; short in stature and childish in appearance" (Osler). The height is usually about three and a half feet. There is marked lordosis, with large, protruding abdomen, and the head is carried well forward between the shoulders. The epiphyses of the long bones are large, the arms and legs are short for the body, and there is genu varum, and with this is often associated a condition of genu valgum. The hands and feet are large and puffy, and the fingers broad and clumsy; the skin of the body is thickened and waxy, often dry and scaly, and seems too large for the body. The hair is often thick on the scalp, sometimes present on the face, but is thin or wanting in the axillæ and on the pubes. Supraclavicular swellings, and often fatty growths, in these and other regions are present. The facial expression is usually dull and stupid, but these little people are easily pleased, and their expression at such times is a smiling one. The eyes are more or less closed by the swelling of the lids and are widely separated. The nose is retroussé, with broad, flat bridge and wide, prominent nostrils. The tongue is large and thick, and in most cases protrudes constantly from the mouth. The lips are thick and prominent, the mouth is large and usually open. The skull is brachycephalic and broad. Asymmetry of the body and head is commonly present. The muscles are weak. The mental condition varies from complete idiocy to a condition of simply feeble intelligence. In the more marked cases deaf-mutism is also present. The memory seems, as a rule, to be good. Epileptiform convulsions are not rare. That there are many grades of cretinism as regards the mental condition

is well recognized, but that there are also degrees as regards the physical development has not been strongly enough brought out. The use of the term congenital myxœdema for cretinism gives us a wider range of classification; and the diagnosis is to be made not alone upon the dwarfed stature and the mental deficiency, but also and principally upon the condition of the skin and connective tissue (Horsley); or, in other words, upon those same evidences upon which we make the diagnosis of the acquired form of the same disease in adults.

These children learn to walk and talk, if at all, quite late in life, and their gait is a peculiar waddle. Dentition is delayed and slow, the teeth rapidly decaying. They have subnormal temperature, and in winter suffer from the cold and are prone to "coughs and colds."

A comparison of the descriptions of acquired myxœdema and cretinism just given shows many points of striking similarity, and this likeness would be more marked were it not for the difference in stature.

As to the etiology of myxœdema but little is known. In adults there is usually a history of some severe drain upon the health and strength of the individual, through severe hæmorrhage, prolonged and severe vomiting as from seasickness, intense and protracted mental worry, frequently recurring pregnancies, etc. In attributing the origin of the disease in any case to a severe hæmorrhage, the tendency to hæmorrhages in these cases, even at an early date, must be borne in mind. In both adults and children the family history is quite apt to show tubercular or neuropathic diatheses in contemporary relatives or ancestors.

The diagnosis of myxœdema in its fully developed condition is not difficult, especially when one has seen even a single case, for the different patients present a remarkable likeness to one another, and the disease has been frequently recognized by intelligent persons, not physicians, from the facial appearance alone. But there is one disease with which the acquired form of myx-

œdema may be, and many times has been, confounded—namely, chronic Bright's disease. In my case previously reported, and in the two first cases of acquired myxœdema given below, a diagnosis of chronic nephritis has been made to answer for all the symptoms for many years, and one case of chronic Bright's has been sent to me as a possible case of myxœdema. In the clinical pictures presented there are many points of similarity, and this is especially true when in myxœdema there are active changes in the kidneys. In both myxœdema and Bright's there is the general swelling, the dyspnœa, the feeble heart, the headaches, the occasional hæmorrhages from the mucous membranes, and the general weakness; and an examination of the urine shows the diminished quantity, of low specific gravity, containing a small percentage of urea and albumin, with usually hyaline and granular, and sometimes even epithelial, casts. The non-pitting nature of the œdema and a careful study of its distribution—for the œdema of Bright's, being affected by gravity as well as by pressure, is found most marked in the dependent portions of the face, body, and extremities, while in myxœdema it is of more general distribution—the dry, rough skin with no sweating, the falling of the hair, the dysphagia, and the subnormal temperature, are some of the most striking features present in myxœdema and do not exist in chronic nephritis. These points, with the manner of speaking and the tone of the voice, will make the differentiation not difficult. While, then, the disease when fully developed presents symptoms which are sufficiently characteristic to be rightly interpreted after an ordinarily careful examination, real difficulty may exist in the early stages of the disease, and a thorough study of all the symptoms presented by the patient be necessary before the diagnosis can be made. The congenital form of the disease in infants in some degree simulates rachitis, but the mistake in diagnosis is not apt to occur.

Treatment.—The treatment of myxœdema by the thy-

roid gland has come to stay, and to Dr. G. R. Murray, of Newcastle, England, belongs the credit of its first practical application. Associated with his name in this matter must be those of Mackenzie, of London, and Fox, of Plymouth, England, who, with Howitz, first used the treatment by the stomach. A very clear idea of the gradual progress in experimental work which led to Murray's consummation can be obtained by reading the "Historical Summary," in Dr. Kinnicutt's [1] late article, where the names of Ord and Horsley are given justifiable prominence. In administering thyroid gland to persons suffering from myxœdema, we supply to them a "something" of which they are deprived by the functional inactivity of their diseased thyroid, thereby causing to disappear most of the abnormal conditions to which such deprivation gave rise. One of these abnormal conditions, however, does not always disappear—viz., the anæmia. It may or it may not be true that a direct hæmapoietic function belongs to the thyroid, but it is a fact that all the other symptoms of myxœdema may be caused to disappear by the use of thyroid extract, and the anæmia of the disease not only not be benefited, but actually aggravated;[2] though in some cases, and notably in Murray's monkey,[3] the red corpuscles and percentage of hæmoglobin are increased. Further experimentation and the use of thyroid extract in simple anæmia will throw more light on this point. The temperature is caused to rise and the pulse and respiration are increased in frequency by the thyroid administration. That this is due directly to the thyroid gland as such, and not to ptomaine or septic poisoning, was well shown by the bacteriological examination made by Dr. Lambert, and included in my former report, of

[1] Transactions of the Association of American Physicians, 1893, p. 314; New York Medical Record, October 7th, 1893.

[2] Putnam: Transactions of the Association of American Physicians, 1893, p. 337; American Journal of the Medical Sciences, October, 1893. Thompson: Transactions of the Association of American Physicians, 1893; New York Medical Record, 1893, xliv., p. 174.

[3] British Medical Journal, September, 1893, p. 23.

the very lot of extract which caused such alarming effects in my first case, and by the fact that when this same extract was administered in smaller but gradually increasing doses no further trouble was caused. The various bad effects of thyroid, noted in reported cases, seem to have been caused by too large dosage. When the hypodermatic method has been used, aside from the occasional abscesses which have been caused at the point of injection, and which are not due to anything thyroid in the preparation, flushing of the face, headaches, indefinite pains, nausea, vertigo, weakness and fainting, are frequently noted, and in two cases [1] epileptiform convulsions have been recorded. When given by the stomach, nausea, vomiting, pains, weakness, profuse perspiration, depression, headaches, rapid heart, and "toxic symptoms accompanied by extreme feebleness of heart," [2] have been observed.

The action of thyroid extract upon the urine in myxœdema is worthy of special consideration. The amount of urine is increased, the specific gravity is higher, the urea is increased,[3] and the albumin and casts disappear. All these changes were well shown in my first case already reported. Ord [4] has pointed out that during the first two or three weeks the nitrogen excreted is in excess of that in the food taken, and that this excretion is mostly as urea. This excretion of nitrogen in excess is well shown by the careful investigations of Vermehren [5] upon four cases of myxœdema, and this observer also found that the thyroid administration had a similar effect upon the urine in senility, though to a less marked degree; and that in myxœdema a much less percentage of the nitrogen taken in the food was found in the fæces, proving the more complete absorption. The observations of Ord,

[1] Murray: British Medical Journal, 1892, ii., p. 540. Hearn: Ibid., p. 452.
[2] Lundie: Edinburgh Medical Journal, May, 1893, xxxviii.
[3] Alexander Napier: Lancet, September 30th, 1893.
[4] British Medical Journal, July 29th, 1893.
[5] Deutsche medicinische Wochenschrift, October 26th, 1893.

Alexander Napier, and others bear out the statement which I have already made, that "at the time when the general swelling was being most actively reduced by the extract the daily amount of urea was increased." This reduction in the body weight is most active during the first two or three weeks of the treatment, and its rapidity will depend upon the amount of thyroid given. The curative effect of the administration upon the myxœdematous symptoms in the adult are known and constant, as the weekly reports of numerous cases show. Two men have, however, reported failures: Mitchell-Clark,[1] two cases; and W. C. Krause,[2] of Buffalo, two cases. By mistake Kinnicutt used a lymphatic gland minced, without any reaction, and Lundie, for a month, used thymus before finding his error, and obtained no other result than elevation of temperature; and I should presume that such mistakes had occurred to others, among whom may possibly be Mitchell-Clark and Krause.

In all the cases under my observation, and in many of the reported cases, the rheumatoid pains in the joints have been aggravated by the treatment; and in two of my cases acute attacks of joint inflammation, with redness, swelling, and severe pain, have occurred, accompanied by a rise of temperature, and these attacks were controlled by antirheumatic remedies. With the exception of the anæmia and the rheumatoid pains, the symptoms of acquired myxœdema are regularly caused to disappear by the use of some preparation of the thyroid gland after from three to four months' treatment. Murray puts the duration of this, his "first stage" of the treatment, at six weeks,[3] and with the hypodermatic method it is possible that this period may be shorter than when treated by the stomach. During this time the patient must be carefully watched, and unusual exertion and mental excitement, and the administration of depres-

[1] British Medical Journal, August 27th, 1892.
[2] Medical Review, September 16th, 1893.
[3] Lancet, 1893, vol. I., p. 1130.

sant drugs, such as pilocarpine,[1] should be avoided, and especially in cases with cardiac or arterial degeneration, and, indeed, in such cases absolute rest in bed is desirable.[2] The result of thyroid treatment in congenital cases is summed up by John Thomson [3] as follows:

1. Growth of skeleton accelerated in infants, and recommenced in older cases. 2. Rapid disappearance of abnormal swelling. 3. Temperature raised. 4. Emaciation and muscular debility during first two or three months. 5. Distinct advance in mental capacity.

After the treatment has resulted in a so-called cure of the disease, much smaller doses will be required to maintain the patient in a condition of health. Cessation of treatment for a few weeks is followed by a gradual reappearance of the symptoms.

The thyroid glands are administered in various ways: when by the hypodermatic method, Murray's original preparation is most frequently used. This consists of a mixture containing one drachm each of expressed juice, glycerin, and a one-half of one per cent watery solution of carbolic acid. Of this from five to fifteen minims are injected two or three times a week. Flushing of the face, or pain at the seat of injection, which is preferably the lumbar region, should be taken as an indication to stop the injection. By the stomach, the feeding of minced raw or partially cooked glands, the powder, and the glycerin extract of various strengths, have been used with success. I have used the glycerin extract in all cases, and make it of a strength of twenty-four grains of gland to one drachm of glycerin. The glands are carefully cleaned, minced, and, after maceration with the glycerin, the mixture is allowed to stand for three or four days, after which it is filtered under pressure as required for use. Full asepsis is observed throughout the different

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[1] G. Stewart: London Practitioner, July, 1893.

[2] British Medical Journal, August 27th, 1892. John Thomson: Edinburgh Medical Journal, May, 1893.

[3] British Medical Journal, September 23d, 1893.

steps, and an anhydrous glycerin, previously sterilized, is used. Diseased glands are discarded. The product should be kept away from the heat and light. The ordinary beginning dose of this preparation, for an adult, is five drops three times a day, to be gradually increased to fifteen drops; in infants, one drop three times a day should be given, and sixteen drops a day, in three divided doses, seems to be the limit of increase. From my experience I believe that the glands of the young animal are productive of more good than those taken from the older sheep, and I now make use only of the glands taken from lambs, using the butcher's guide of ununited epiphysis to determine the approximate age. The cases reported below are significant in showing this difference. The extract thus made will keep for months without deterioration or apparent change.

The histories in the first two cases reported below have been very generously given to me by Dr. Starr, to whom the patients had been sent.

CASE I.—Mrs. B., aged fifty-five years. Since the birth of last child, fourteen years ago, she has been suffering from present disease, and at that time was subjected to much mental worry, caused by the death of two children. Three weeks after this last delivery she was badly frightened. The first symptom which attracted attention was the swelling of the abdomen, and later came metrorrhagia which was severe enough to cause anæmia. Menopause occurred ten years ago. About seven years after last accouchement, on awakening in the morning, the whole body was swollen, and shortly after this the skin showed irritation and became dry and scaly. She has had twitching of right arm, and at one time para-anæsthesia of right side; no paralysis, no hallucinations. The disease had been gradually progressive until her appearance at Dr. Starr's office on May 13th, at which time I first saw her. At this time the patient presented all the characteristics of a typical myxœdemic, in the swelling, skin, speech, dysphagia, dyspnœa, anidrosis, falling of the hair, the subnormal temperature, dulness of hearing, and hæmorrhages from mucous membrane of mouth, and in her mental condition. There were slight hectic spots over

malar bones, but no chloasmata, and pain along sterno-mastoid muscles and in the ankles was complained of. The patient was not confined to bed or house, but was weak. Arch of eyebrows was characteristic. Anæmia was present, but not marked. This patient was immediately put upon the glycerin extract of thyroid gland of sheep (gr. xxiv.–3 j.), average weight of one lobe one hundred and eighty-nine grains; gtt. viii., t. i. d., to be gradually increased. Three days later a change in the voice and in the physical appearance was perfectly apparent, and the patient herself said that her clothes were looser and that she felt better. No rise in the temperature, either immediate or remote, was induced, and, taken by mouth, was 96.5° on the third day.

June 7th. Free perspiration occurred, and the skin was somewhat less rough. She had now reached a dose of gtt. xiii., t. i. d.

June 12th. The patient was taking gtt. xx., t. i. d. The improvement was slow, but progressive; general swelling was much diminished, finger rings slipping off easily, and there was no dysphagia. Dyspnœa only on exertion.

July 26th. Patient had reached gtt. xxiv., t. i. d., and had had no rise of temperature much above 97°, but on this date temperature rose to 100.5° and she suffered severely from rheumatic pains, especially in knees. Salol and phenacetin somewhat diminished pain, which, however, lasted for about a week, and thereafter frequently recurred. Hair continued to fall. Improvement slowly progressive.

August 2d. I put the patient upon an extract made from the glands of lambs, average weight twenty grains to the lobe; gtt. x., t. i. d., were given.

September 1st. Since the change to the young glands the improvement has been rapid. Swelling diminished much more sensibly; hair ceased falling; exfoliation took place from whole body, and skin was soft and moist, and speech more nearly natural. Physical and mental strength greatly improved. During the following week the patient was without extract, and at the end of that time, though showing no signs of returning disease, was much prostrated. Extract was again begun and pushed rapidly to gtt. xv., t. i. d.

By the middle of September the patient showed a change that was marvellous, and was apparently in a per-

fectly normal condition, with the exception of the anæmia, with a temperature of 98.8°. She was directed to diminish the amount of extract to gtt. xv. twice a day, but at the end of two weeks complained that she felt that this was too little, and it was again increased to gtt. xv., t. i. d. This dose has been gradually diminished, and the patient remains well.

CASE II.—Mrs. C. A photograph of this patient taken in 1881 shows a normal face with features clear and sharp, but with some slight fulness of the upper lids. The hair at this time was abundant and nearly white. The menopause occurred in 1883. Early in 1884, during an ocean voyage, this patient was badly frightened and dreadfully seasick, perspiring most profusely. Later in this year the myxœdematous condition was well marked, the skin being dry and without perspiration, and this was the first symptom noted by the patient. Hands became very dark, almost mahogany color. She has had slight dysphagia since a girl, but after 1884 this became worse and was distressing. By the end of 1884 the slowness of speech and the swelling of the face, neck, body, and extremities were well advanced. During the year 1890 she was attacked once with sudden loss of consciousness and fell. In January, 1892, she had pneumonia, which ran a regular course. Dr. Starr saw her first in May, 1891, at which time she was in a condition of chronic hysteria, with the condition of myxœdema well advanced, the eyes being nearly closed. The urine showed sp. gr. 1015, no albumin, no sugar; pale, cloudy, and with granular casts; the daily quantity was thirty-two ounces. On June 14th, 1893, Dr. Wm. H. Thomson referred her to Dr. Starr for treatment by thyroid extract. At this time the diseased condition was far advanced and the general swelling marked. The mental symptoms were especially prominent; melancholia, apathy, hallucinations, and vivid dreams existed; marked dysphagia but no dyspnœa. Eyes nearly closed and characteristic arch of eyebrows present. Temperature 97° and 98°; constipation. She was practically confined to her room. Thyroid extract from glands of old sheep, with average weight of each lobe one hundred and eighty-nine grains, was given, beginning dose being gtt. vi., t. i. d.

July 6th. Dose of extract was at this time gtt. x., t. i. d., and although the temperature had at times risen to 99.2°

and was uniformly more nearly normal, the condition of the patient was practically unchanged. She was now given gtt. vii., t. i. d., of an extract which, though seven months old, was made from young glands averaging nineteen and a half grains for each lobe.

July 19th. Since the change that was made in the extract ten days before, the constant elevation of temperature to above 99° has allowed of only small doses of the extract, varying from five to twenty drops daily, and notwithstanding these small doses the improvement has been marked: the swelling of the face and body much reduced, the tongue and lips much less swollen and more mobile; she can pronounce "st." The dysphagia was less marked and the patient herself began to have hope. The skin was more natural, and sweating occurred on warm days. Pain was still present in the knees and very severe at the end of the spine. Her general strength was not much improved.

August 1st. Extract, gtt. v., t. i. d. Temperature, morning, 97.5°; evening, 99.6°. The improvement had been gradually progressive, and she felt in better health and spirits.

September 1st. Extract, gtt. xvii., twice daily. Temperature, morning, 98°; evening, 98.5°. Very much improved and took short walks in the open air.

September 13th. The patient came into town and presented herself at Dr. Starr's office, and at his invitation I was present. Mrs. C. now looks, talks, and acts like a normal woman in good health. She is bright and cheerful. There was some stiffness and slight swelling still existing in the upper lip; her hands were small, and her whole skin soft and natural. She felt strong and well, and younger by many years, and looked so. She occasionally stains her pillow with the bloody mucus from the mouth. She occasionally had rheumatoid pains, and the anæmia was not improved.

Case III.—Mrs. D., aged fifty-six years. The family history in this case is entirely negative. The patient had had only two children, both dying in infancy. Severe hæmorrhage followed the birth of the last child, twenty years ago. Rapidly increasing paresis of all the muscles began ten years ago, and was the first symptom of the disease, and this became so pronounced that she walked with difficulty and could not hold up her head, which fell forward. The swelling soon came on, and with it darkening

of skin of hands and face. For the last five years the skin has been dry and scaly and thickened, and all the other symptoms of the disease have gradually appeared, and to-day the picture presented is characteristic. There have been periods of improvement without known cause, and she is better in warm weather. She is very feeble and looks like an old woman, and in this case the analogy to senility pointed out by Vermehren is striking. Treatment was begun on October 19th. Extract of lambs' thyroids (gr. xxiv.-3 j.), in doses of five drops, t. i. d.

November 1st. Improvement already manifest and patient feels stronger. Mind less confused and memory better. Dysphagia nearly gone. Extract in doses gtt. x., t. i. d.

November 10th. Swallowing, speech, and hearing nearly normal. Desquamation ceased and discoloration fading from hands and face. Paresis disappearing and has discarded cane. Swelling much less. Hair ceased falling, and upon scalp new hair coming. Anæmia not improved. Extract, gtt. x., t. i. d., continued.[1]

CASE IV.—D. D., female, aged five years. Born in Boston, of New England parents, who are persons of more than ordinary intelligence. No history of thyroid disease in the family of either parent. Mother partial invalid from neurasthenia for some years, both before and since marriage; she has irritable and rapid heart, is somewhat anæmic; her eyes are large, but exophthalmia is not present. During this pregnancy, her first and only one, she was ill for five weeks with tonsillitis, and during the second month she broke the third rib on the right side by an accidental blow from a hammer. No special worry was present, but vomiting persisted during the entire pregnancy. Delivery was instrumental and under ether anæsthesia, and the cord was around the neck. At birth the child weighed eight pounds and seemed normal in action and appearance. Nothing was noted until at three months, when the tongue seemed thick and the baby was pronounced tongue-tied. Her weight was then fifteen pounds and she seemed a normal child with the exception of the tongue and a state of chronic constipation. When four months old she cried at night a great deal and suffered from attacks of spasmodic dyspnœa. At six months it was noticed that the child had ceased to grow and was

[1] November 22d, cerebral hæmorrhage and complete hemiplegia.

losing in weight, and this was attributed to the nursing of the child by the mother, who was not at all strong. When eight months was reached it was appreciated that the child was far from normal, for the swelling of the face, the protrusion of the large and swollen tongue, and the lack of mental and physical development were painfully apparent to all. At eleven months the child was weaned, and the dysphagia was so marked as to necessitate feeding with a dropper. When two years old the two lower incisors appeared, but were small and soon discolored. At three years the general swelling was at its worst. During her fourth year epileptiform convulsions were frequent, some days as many as a dozen.

Last August the child came under the observation of Dr. G. F. Merrill, of Kennebunkport, Maine, and then for the first time the condition was recognized as being myxœdema, and I was asked to see the patient. The condition, both mental and physical, presented was that characteristic of the disease. Though five years old the child was of the size of a ten months' infant. The lordosis and the large, prominent abdomen were especially marked; and the tibiæ were quite bowed in the lower third. The swelling of the face, nearly closing the eyes, the marked mental apathy, and the protrusion of the tongue gave the face the appearance of absolute intellectual vacuity. But on closer examination it was found that some degree of intelligence did exist, though the power of mental perception was very slow. Her attention was attracted with difficulty, and even a lighted match moved in front of her face was not followed by the eyes. Only loud noises would cause her to turn, and these she seemed unable to locate. No expression of face or body showed any response when she was called by name. Her disposition was good. When placed upon her back upon the floor, she would, after many ineffectual attempts and much labor, succeed in turning over on to her face and belly, where she would lie exhausted by the effort. When set upon the floor she seemed incapable of maintaining the balance of the body, and would topple over without any effort to save herself or any fear as to the possible consequences. The condition of the skin was marked, and the hair on the head was thin and coarse and was also present upon the forehead and sides of the face. The axillary temperature ranged between 97° and 98°. A considerable degree of anæmia existed. Upon September 1st the

treatment with extract of lambs' thyroids, gr. xxiv. to the drachm, was started with a beginning dose of one drop t. i. d. This was gradually increased until four drops were given and the temperature caused to rise to 99°. Within a week a letter received from Dr. Merrill stated that a change for the better was manifest, and on September 16th the father informed me that the appearance of the child was much improved and that she "noticed more." Upon September 27th the child was brought to New York to remain under my personal observation. At this time five drops t. i. d. were taken and the temperature was normal. The swelling of the face and body was much reduced, the lips less prominent, and the tongue smaller and much more movable, and could be kept within the lips but not within the closed jaws. The skin was much softer and not so dry. The bowels were normal. She was certainly brighter, and looked up into the face of any one approaching, and when her name was spoken would turn her head in the direction of the voice.

On the 5th and 6th of October the temperature, which had been gradually rising upon fifteen and sixteen drops daily in three divided doses, reached 101° at midnight; and on the 7th the dose was reduced to three drops three times a day. The improvement in the child's condition, bodily and mentally, during the previous ten days had been remarkable. Her appetite was much increased and her bowels were normal. The swelling of the face, body, and extremities was much reduced, and the natural lines of the face were beginning to show. The urine was much increased in amount. She did not sleep so much nor so well since the beginning of the treatment, and she cried more.

October 16th. The patient had continued to improve steadily, and at this time the manifestations of the disease which still remained were those which it takes time to grow away from. Her mental aspect had improved even more than her physical condition, and on account of the increased mobility of the features, and the greater bodily activity displayed, her intellectual processes were more apparent. She looked with interest on surrounding objects, and when held before a third-story window followed with her eyes the passing persons, horses, and even dogs. She made her wants better known, kicked her legs about, and when lying upon the bed her arms and legs would be in constant motion. When put upon the floor, with a

little coaxing she would roll over and over as many as ten times. She used her hands as prehensile members, and she showed a disposition to learn to walk, but I discouraged this until her body should weigh less and her legs be stronger, because of the tendency to bowlegs. Five teeth have appeared, and they seem sound and good. Only twice has she had convulsions since the beginning of the treatment. The following comparative measurements are of interest as showing the general diminution in the bulk of the body and the increased growth between the two dates given—a period of about seven weeks:

	Sept. 27th.	Nov. 14th.
Circumferences—	Inches.	Inches.
Skull: Occipito-mental	21½	21
Horizontal	19½	18⅞
Face	19	18
Neck	12¼	11
Right arm	5¾	5
Left "	5½	5
Right forearm	5½	5½
Left "	6	5½
Left thigh	9½	9
Left leg	7	6¾
Upper chest	19¾	20
Lower "	21½	20
Abdomen	19	19½
Lengths—		
Right arm	4	4¼
Left "	3⅝	4¼
Right forearm	4	4½
Left "	4	4⅜
Lower extremity, left	10½	11

We must bear in mind that in treating congenital myxœdema we have not a pre-existing condition back to which we can bring our patients, but in these cases the first step in the treatment is to restore the health, and the second step is to maintain the child in a normal condition, so that the development of its mind and body will progress as in any other healthy child of the same apparent age. That such development will take place I feel sure, but, until the cases have been kept under observation and treatment for some years, absolute proof cannot be had. It is my belief that if we break away from the term cretinism, with its vivid picture of the "pariah of Nature," and look more closely for the symptoms of myx-

œdema, we will find that many so-called idiots, imbeciles, cases of arrested development, etc., among children, are in fact cases of functional inactivity of the thyroid gland, and hence susceptible of treatment by thyroid extract, with improvement, and perhaps even cure.

The effects of thyroid administration may be summed up as follows:

Increased metabolism; shown by—

1. Elevation of temperature.
2. Increased appetite, with more complete absorption of nitrogenous foods.
3. Loss of weight, with nitrogen excreted in excess of that taken in the food.
4. Growth of skeleton in the very young.
5. Marked improvement in body nutrition generally.
6. Increased activity of mucous membranes, skin, and kidneys.

The rheumatic symptoms and the anæmia are not only not relieved, but are most frequently aggravated.

DISCUSSION.

DR. M. A. STARR.—I will simply allude briefly to one or two of the clinical features of myxœdema that Dr. Crary has not spoken of, although he has left very little for any one to say on the subject. He alluded to the fact that mental disorders are not at all uncommon in the course of myxœdema. This has been noticed by numerous observers, and I believe forty-six per cent of the cases reported suffered at one time or other with mental disturbances. It was on account of their mental condition that three out of seven cases of myxœdema were sent to me. The prevailing type of mental disorder is not that of any of the ordinary forms of mental disease. The patients are melancholic, but they have not many of the characteristics of melancholia. They are demented, but they lack many of the features of pure dementia. There seems to be rather an apathy of mind with a tendency to depression, without fixed delusions, and with inability for connected thought. The condition corresponds closer to dementia than any other form of insanity, although it differs radically from the ordinary dementia. I speak of

this because I have no doubt that when the fact becomes well known that this form of mental disturbance is present in these patients, it will be discovered that there are many persons at present confined in insane asylums who are suffering really from myxœdema and who are entirely curable by means of thyroid extract. No one who has seen the effects of this remedy can wonder that the subject has excited a great deal of interest, and I hope, by means of the photographs I have to show you, to give you a vivid picture of the appearance of these patients, so that you will recognize them whenever you see them. I speak feelingly on this subject, because three of these patients, who are now perfectly well, were under my own care for a considerable period of time before I recognized the fact that they were suffering from myxœdema; I did not recognize it until I had seen a typical case of that disease. Three of the cases of which I am about to show you photographs were seen by me and by a number of other physicians, and were diagnosticated as chronic Bright's disease. I believe that many so-called lunatics and many cases diagnosed as Bright's disease are in reality cases of myxœdema. For the purpose of making this clear I have had these photographs prepared; some of them were kindly loaned to me by Drs. Thompson, Draper, and Crary.

Dr. Starr then showed twenty photographs of ten patients suffering from myxœdema, demonstrating the clinical appearances characteristic of the disease and the remarkable changes in appearance produced by treatment.

Dr. W. G. Thompson.—The therapeutic use of thyroid extract is, to my mind, one of the most remarkable discoveries of modern medicine. It is of very great interest both from a clinical and physiological point of view. We have heretofore known practically nothing of the use of the thyroid gland. We are aware that it has a most abundant supply of vessels and nerves, but the most careful chemical research has failed to explain its real value. We find in its structure chemical compounds which are common in other organs of the body, and for this reason it seems very strange that it should be able to influence the body nutrition to such a degree. It is equally potent whether given by the stomach or by the hypodermic needle. The physiological experiments that have been made with the ingredients of this gland have not been altogether convincing and have differed widely from one

another. The extirpation of the gland in the lower animals results in the production of certain symptoms referable largely to the nervous system. There is more or less tremor, and inability to use the voluntary muscles. The animals ofttimes become dull and stupid, and later on changes occur in the general body nutrition. These symptoms do not always follow the removal of the gland. Occasionally the health of the animal seems to be maintained, and in some of the lower animals, like rabbits, birds, etc., the gland may be removed without any disturbance being produced. Its effect on the body nutrition of man, therefore, is exceedingly interesting; and when we consider that we are dealing very often with a disease which has existed for twelve or fifteen years or longer, during which period the symptoms were steadily progressive, and wherein the mental symptoms are so marked and peculiar, the effect of the clinical use of the thyroid extract becomes even more astonishing.

The case of myxœdema that came under my observation and treatment was a typical one. It had existed about twelve years. The patient was a female. She came to the Presbyterian Hospital last April, and was immediately put upon treatment with the thyroid extract. The preparation was at first used with great caution because of its marked reaction. There were, however, no untoward symptoms, no elevation of temperature, no cardiac disturbance. Within the first ten days decided improvement occurred. The improvement in the mental symptoms was very marked. On admittance to the hospital the woman was very dull and listless; she was despondent about herself, she was unable to sew or to read, and took no interest in anything. There was œdema of the face and neck and of the upper and lower extremities. The skin was dry and exfoliated to an extraordinary degree, somewhat like the scaling in scarlet fever, and the hair was falling out. The patient was admitted to the hospital in April, 1893, and discharged in August, practically well. She is now living on Long Island, and in a communication received from her two days ago she states that she is in good health, her appetite is good, and that she is able to attend to all her household duties. With the improvement in the other symptoms the skin and hair were also much improved, returning to their normal condition.

A few days ago I heard of a curious fact in regard to

one of the cases of myxœdema, one of Dr. Kinnicutt's patients. The patient had lost nearly all of her hair, and when it was restored under the treatment by thyroid extract it came back quite black in color, having previously been gray.

Dr. W. K. Draper.—Photographs of the patient whose case I have to report have been shown by Dr. Starr. The history is as follows: Mrs. B., thirty-seven years old, married, came to the Vanderbilt Clinic in June, 1893. Family history negative.

She was in good health till the beginning of her present illness. She has had six children, four of whom are living. About five years ago, shortly after the birth of her last child, she began to suffer with pain in the back, and noticed that she was losing strength. She had "no ambition" and was unable to attend to her household work. She said that her tongue felt thick and that her speech became slow. Her hands and feet became numb and began to swell; the abdomen was also swollen. She always felt cold, had some pain in the left side, palpitation of the heart, dyspnœa, mental depression, and noticed a diminution in the amount of urine passed. She was seen by several physicians and treated for Bright's disease. The above symptoms have continued, and existed at the time of her first visit to the Clinic.

She admitted, on questioning, that she never perspired, was languid and sleepy most of the time, and could do no work on account of her weakness; also that she was emotional and irritable, and wanted to be quiet and alone. Any bodily or mental exertion was an effort for her, and she had occasionally fainted.

Her appetite was poor and her bowels constipated. On examination she presented the following appearance: Large; fat; very anæmic; hair dry and brittle, absent in axillæ; skin of face, hands, body, and legs dry and scaly; no perspiration; face swollen, especially under the eyes; lips and tongue thick; gums spongy, bleed easily; teeth bad and loose; hands thick and swollen; fingers clumsy; some hard œdema of feet and legs; few slight varicose veins. Temperature 99°, pulse slow. Urine acid, 1013, cloud of albumin, no sugar; no microscopic examination made. She was put on the glycerin extract of sheep's thyroid glands, gtt. viii. three times a day. She returned in ten days; had had no bad symptoms from the medicine; felt better, was warm, had begun to perspire, and had more energy. Her gums looked less spongy.

A month later she was much stronger and was passing an increased amount of urine, which was examined: it was acid, 1016, cloud of albumin, no sugar; on microscopic examination a few epithelial cells were found, but no casts.

She continued to improve until early in September, when, owing to the fact that she feared she would be exposed to small-pox at the Clinic, she absented herself for more than one month. On returning she complained of the old feeling of weakness and loss of energy. She was again given the thyroid extract, and again improved. On November 1st the skin was soft, hair soft and shiny; speech clear and no longer slow; gums much better, though her teeth are still somewhat loose.

She is now able to do her own work, and has walked a mile without stopping.

DR. R. B. KIMBALL.—I have had a single case of myxœdema under my care. She first came under observation last summer, when she presented a number of interesting symptoms. There was pronounced disturbance of the skin, apparently of vaso-motor origin. The patient was fifty-five years old. Her health had been good up to the date of her present illness, which commenced five years ago. The skin was thickened, its sensibility impaired. Her hair had entirely disappeared. Her mental condition had undergone deterioration. She had heavy, overhanging eyelids, giving the eyes a half-closed appearance. The ears were massive in size, the lips thickened, the tongue seemed double its natural size. I am not positive, but I think I could feel the thyroid gland. The hands were so much swollen that she could not approximate her fingers. The speech was slow and drawling. She did not perspire at all. The skin was insensitive, and flies lighting on her face produced no sensation whatever. She had twice been severely burnt by a mustard plaster without feeling it. All her acts were performed slowly and listlessly. She seldom took any exercise, and spent most of her time sitting in bed swathed in flannels. Her mental condition was poor. Her temperature was subnormal, pulse 100. The urine was of a low specific gravity.

Treatment by means of thyroid extract was commenced August 2d, 1893, and at the end of one week the improvement in her condition was noticeable. At the end of the second week her facial expression was much better.

There was a perceptible growth of new hair, brown in color, from the eyebrows. The patient began to perspire freely. The pulse averaged 105. The temperature was 98°. Emaciation was very marked. Within four weeks after the beginning of treatment the patient lost forty pounds in weight; the skin became soft and moist, and the ears and tongue decreased in size. On September 13th the patient is able to take considerable exercise, and the change in her appearance is startling; her clothes hang loosely about her, and there is a fine growth of hair on the scalp. September 22d: The patient looks well, is vivacious and able to do her regular work; pulse 85; specific gravity of urine, 1020. November 13th: Hair an inch long and very thick; walks a mile every day; has taken long railway journey alone; eats and sleeps well; is cheerful and vivacious; presents no trace of myxœdema.

Dr. J. W. Brannan.—I would like to inquire as to the permanency of the cure. Will it be necessary for the patients to continue taking the extract as long as they live?

Dr. H. W. Berg.—Dr. Starr promised that Dr. Thompson would tell us how to make the differential diagnosis between the œdema due to Bright's disease and that due to myxœdema. I regard this as a very important point, and would like to hear further from Dr. Thompson on this subject.

Dr. W. G. Thompson.—I thought that ground was already pretty well covered. The œdema in myxœdema does not readily pit on pressure. In Bright's disease the œdema is commonly diffuse, while in myxœdema it is more apt to be localized. In the latter disease, too, there is a peculiar wrinkled condition of the skin over the eyes; in Bright's disease the eyes can be kept open more easily. The two photographs shown by Dr. Starr, one of myxœdema and the other of chronic Bright's, clearly illustrated the marked contrast in the character of the œdema in the two diseases.

Dr. Crary (closing the discussion).—I wish to add a few words as regards the temperature reaction after the administration of thyroid extract. The extract made from the glands of old sheep did not give a marked rise in temperature, while that made from the lambs' glands effected a distinct temperature reaction. In my case of congenital myxœdema I could not give more than three drops of the extract three times daily; this solution con-

sisted of twenty-four grains of the gland to the drachm. The above dose kept the child's temperature at about 99°. An increase of one drop will increase the temperature one or two degrees F.

In almost all cases of myxœdema the kidneys are secondarily involved, so that you may have, coincident with the original affection, a condition of chronic Bright's disease, and on this account it may be difficult to differentiate the œdema of one from the other. In Bright's disease the œdema is not only affected by pressure, but also by gravity: you find it more pronounced in the lower than in the upper extremities.

One word about the rapidity of the treatment. You can undoubtedly shorten what is called the first period of the treatment, but it is not of advantage to do so. Three deaths have been reported due to pushing the treatment. The same good results can be obtained without the pronounced weakness and without the marked elevation of temperature by giving small doses and taking more time.

As regards the continuance of the treatment, we do not cure the disease by means of the thyroid extract. The symptoms are only caused to disappear by supplying to the system something that it lacks on account of the functional inactivity of the thyroid gland. Consequently the treatment must be kept up for all time.

Anniversary Discourse, November 29th, 1893.

MODERN EXPERIMENTAL MEDICINE.

By WILLIAM H. THOMSON, M.D., LL.D.

Mr. President, Fellows of the Academy, Ladies and Gentlemen: No physician who has lived long enough in the practice of his profession to compare the state of medical knowledge and of opinion as it was twenty-five years ago with the same at the present day can fail to recognize the fact that in both a virtual revolution has characterized the interval. This change is not simply from increase of knowledge, great as that has been, but more from change of methods in the pursuit of knowledge, with the result of altering the whole domain of medicine to an extent which only the term revolution can fully express. It happens, however, that, as in other revolutions, many who live in their epochs, and who even may take part in them, yet fail to appreciate at the time their full significance, and therefore remain long unaware of the essential difference which separates the new order of things from the old. With a view, therefore, to illustrate the import of the new era which is now fully upon us, I would briefly refer to the chief methods of inquiry and of investigation which were pursued by the medical profession a quarter of a century ago, and then compare them with the experimental medicine of to-day.

First of all in the estimation of the profession at that time came clinical medicine, or the knowledge that could be gained of disease at the bedside of the sick. To be able to investigate disease to good purpose when face to face with it required no small preparation on the part of the physician. For that preparation anatomy was the

first lesson, and no complaint can be made that this essential prerequisite to any true medical science was then, or ever has been, underestimated. No sooner, in fact, was the microscope put within reach than everywhere workers set themselves to extend our knowledge of the structure of living tissue, until the vast field of histology, or the ultimate anatomy, as it may be termed, was explored in every direction, with the most fruitful results, though thereby more than doubling the amount of study now required for even ordinary medical equipment, compared with that of previous generations. With each advance in knowledge of structure it was to be expected that similar gains would be made in knowledge of function or working, and physiology accordingly kept even pace with anatomy both in the extent and suggestiveness of its discoveries. But life has also its own chemistry, and no charge can be made against the profession that it failed to recognize the importance of that great science in its relations to health and to disease.

With as perfect qualification in these respects as the student could obtain, diseases then came to be studied in works treating of their general and of their specific forms, or their natural history, so to speak, as revealed in their symptoms, course, and terminations. Finally, to complete this came the great science of pathological anatomy. From ancient times it has been felt that death by disease had many explanations to offer for itself in the records which it leaves of its ravages in the body, and no one can speak too highly of the patience and skill with which unnumbered scientific men in our profession have labored to trace the minutest steps of deviation from healthy structure which the post-mortem table can reveal, and to determine their relation to morbid processes before death.

With all these requisites mastered, clinical observation began. For this also a special training was needful. After an inquiry into the history of the patient, involving often a cross-examination as special in its way as that of

a skilful lawyer, though as far as possible from a lawyer's usual intentions, the clinician begins to observe for himself. The great physicians of antiquity admirably had paved the way by showing how much the eye alone could note in disease, and it would be well for us in this generation to go back to those old masters for many a now too often neglected lesson. But modern research has added the accurate sense of hearing in the methods of percussion and auscultation, and refined many of the applications of the sense of touch, and finally called in the invaluable aid of chemistry and of the microscope for the examination of the secretions—all to guide the medical man up to the point when he will show what his own mind is capable of toward correctly estimating the relative importance of each, among often a wonderful complex of details.

Now, why is all this not enough? Why is this trained investigation of disease, with all its helps for the interpretation of its manifestations or of its issues, whether in recovery or in the anatomical changes shown by autopsies, not sufficient to advance medical knowledge until every serious problem of medicine be solved? The answer is that by none of these methods can the vital question of etiology, or the causation of disease, be answered with anything like a satisfactory demonstration, owing to the absence of one great requisite in all scientific demonstration—namely, experiment. In no scientifically conducted investigation is observation alone sufficient. Observation never can be safely interpreted except by testing. All interpretations of observations remain mere theories until they are experimentally tested. But observing over and over again is not testing; it still remains observing. How deficient, therefore, both clinical medicine and pathological anatomy may be in securing the final interpretation of disease one example will suffice to illustrate:

No malady has been more thoroughly studied, either at the bedside or post mortem, than pulmonary consump-

tion. Both its distressing clinical features and its fearful mortality furnished the strongest incentives for its exhaustive investigation, while its chronic course gave ample time to note everything connected with its development. For centuries, therefore, it has been closely watched, till not one of the minutiæ of its characteristics escaped observation. But to what conclusive results did all this clinical and anatomical study attain? Not to go back too far, I remember a sagacious and acute observer among my teachers eloquently advocate the view of *his* great teacher, Laennec, that tuberculosis was a constitutional tissue degeneration, so hereditary, therefore, that, as my professor expressed it, it began in the ovary of the mother. But whereas Laennec said in 1819, and said rightly, that the existence of tubercles in the lungs is the cause and constitutes the true anatomical character of consumption, the eminent German teacher, Niemeyer, in 1867, said wrongly that tubercles had no causative connection with phthisis, and when found in the lungs after death they were inflammatory products of recent origin which only complicated the disease after it was already in an advanced stage. Louis, the successor of Laennec, and his followers taught that the yellow or cheesy masses of degenerated tubercles were the true tubercles, and were deposited as such from the blood. Virchow, on the other hand, came near denying that cheesy deposits in consumptive lungs had anything to do with tubercle.

Now, no one can allege that these distinguished men whom we have mentioned were either incompetent clinicians or pathologists. Can any better than they be expected in the future? If not, then how long would the profession have had to wait for a final agreement among such authorities on this subject? From what has developed since, the answer would be: Forever. Far as clinical observation can go, it could never have shown the cause of consumption on a wall, or have forbidden floor-sweeping on account of it; and as to pathological anatomy, while we think Sir Andrew Clark's remark much

too sweeping which he makes in his last published lecture on fibroid phthisis—viz., that "the anatomical structure of a morbid product can *never* be made the true criterion of the disease which produces it"—yet pathological anatomy had been given as full time and opportunity as clinical observation to discover the true etiology of phthisis, and had equally failed.

The first real advance toward the solution of the problem was made, as is well known, by Villemin in 1865, when he startled the medical world by announcing what was really a confirmation of a forgotten experiment made as far back as 1843—viz., that tuberculosis could be caused in perfectly healthy animals by inoculating them with tuberculous matter from persons sick with consumption. Experimenters in all countries speedily confirmed this epoch-making discovery. But upon this followed a crowd of most puzzling reports of experiments, which, however, most instructively illustrate the intrinsic excellence in the long run of the experimental method, for at first it seemed as if the experimental investigation of the nature of tuberculosis was going to lead to greater confusion than ever. Dr. Burdon Sanderson found that he could produce tuberculosis in animals by inoculating them with pus taken from pyæmic patients, or even after the mere introduction of a clean seton of unbleached cotton. Dr. Wilson Fox did the same in guinea-pigs by inoculating them with vaccine lymph. In Germany, Cohnheim and Fränkel also succeeded in producing tuberculosis by introducing all sorts of things, from pieces of healthy muscles to bits of charpie or gutta-percha. The inference, therefore, seemed justifiable that anything which artificially produced an inflammation could give rise to tuberculosis, much to the delight of Niemeyer. Ere long, however, it was discovered that it altogether depended upon *where* the animals were at the moment they were experimented upon whether they became tuberculous or not. If they were taken out of cages or rooms in which other animals had died of tuberculosis, and were

then wounded, especially with instruments which had been used on tuberculous subjects, they contracted tuberculosis, no matter what was used. But if precautions were taken against any contamination of the kind with tuberculous matter, then they never became tuberculous until they were inoculated with actually tuberculous material. On the announcement of this fact by Klebs, one by one of the other experimenters mentioned repeated their experiments and finally acknowledged that Klebs was right, until a general agreement was reached, as demonstrated by experiment, that tuberculosis is a specific infectious disease which never occurs in an animal body except it comes from a previous case of tuberculosis.

We have said that this history illustrates the intrinsic excellence of the experimental method; for while at first, in this instance, it scarcely seemed to settle anything, it finally did settle what it set out to do conclusively. This the experimental method does by providing by its own laws for the correction of its own mistakes. Mistaken experiments, unlike mistaken doctrines, are always short-lived, and we commend this fact to all those who dwell upon the contradictory reports of experimental investigation in many questions now before the medical world, as if certainty can never be thus reached. From its very nature the experimental method gets at its hid treasures by no lucky find in the field, but only by the slow approaches and perfecting which mark every human invention, taught often as much by failure as by partial success, and content with nothing short of complete solution. The reason of this last characteristic is that the solution is never left to one person or judge. Every claim of the kind is passed upon by a very large jury of experimenters in all civilized countries, each one of whom tests the solution by experimenting upon it himself. Only what is true, therefore, can survive such an ordeal.

But to return to tuberculosis. Experimental medicine could not stop with the confirmation of Villemin's discovery. One discovery made, urgently called for a further

discovery. As tuberculosis is an infectious disease, what is the infecting agent? Among the many who set themselves to answer this question, it fell to Robert Koch to make the identification, by the strictest rules of experimental investigation, which, as he formulated them at this crisis in his own personal history, illustrate the noble quality of scientific conscience quite as strikingly as his work did that of scientific skill. No investigator, he said, can profess to have demonstrated the causative connection between an observed element in a disease, when it is a micro-organism, and the disease itself—

1. Unless the micro-organism is present in all cases of the disease in question.

2. It must be present in this disease and in no other.

3. It must be found in such quantities in the infected body that all the symptoms of the disease may be clearly attributable to it.

4. It must be capable of isolation from the infected body, and of being separately cultivated where it cannot be confounded with any other micro-organism.

5. These artificially-grown colonies must then be capable of reproducing themselves in animal bodies after inoculation with them.

6. They should then cause just the same disease in symptoms and in anatomical changes as the original growth.

Now, these rules plainly imply that all experimental investigation into the facts of life and of disease is beset with fallacies—fallacies partly inevitable, and partly due to the natural eagerness of inquirers to make striking discoveries. But by such rules an effective check is imposed from the first upon any straying from the path of testing and of retesting. The experimenter has to stop and wait for five other corroborations of his first experiment ere he can certify demonstration. Then at every step he must report, not his views or his conclusions, but instead fully detail his methods of working. Room for views or opinions there is little or none. From first to last nothing is considered but questions of fact.

It is well that this should be so, for when these demonstrated facts were first reported about tuberculosis, owing to their extraordinary and well-nigh unimaginable character, they would have met with nothing but general incredulity were it not that all evidence, when it is fully certified by experiment, is simply resistless. A lung stuffed with tubercles must swarm indeed with Koch's bacilli when, according to Bujwid's estimate, 5,000,000,000 of them can enter a drop of water. Still, minute as they are, they are not dissolved in it, for they can all be filtered out of it as so many solid particles. Moreover, a whole series of facts have been further demonstrated about them which it is of supreme importance for us to know. First, they are living things, with as definite a life history as other living things. They can be cultivated in or on different media outside of the body, and generation after generation of them can so be raised, with full opportunities thus given for watching all their facts of growth. Koch still keeps propagating in his laboratory cultures descended from the first which he planted in 1881, just as a farmer might do with his different kinds of grains and thus know the difference between them when grown on this soil or on that. Thus it seems that the range of temperature under which they will grow is more restricted than with other micro-organisms—37° to 39° C. being the best. Nevertheless they are not killed by freezing any more than winter-sown wheat. On the other hand, whereas the cholera bacilli are quickly destroyed by drying, tubercle bacilli will retain their vitality in dried sputum for nine to ten months; hence the deadly peril from the dust of rooms in which consumptives have carelessly expectorated. Moreover, these bacilli have great powers of resistance against certain germicides which are promptly fatal to other bacteria. They are not destroyed by the gastric juice, and hence, when taken in meat or milk, will pass on to invade the system through the abdominal walls. Nevertheless they are extremely susceptible to direct sunlight, which kills them in a few

minutes, and even in the diffused light of a room with good windows they will die in six to seven days. We need not fear, therefore, the consumptive's expectorating on the sidewalk, however careful we should be about him indoors.

Already, therefore, we are in possession of such priceless information as this: First, that this fell destroyer of the human race is a preventable disease, and therefore we may confidently look forward to the time when, instead of its being one of the commonest causes of death, it will become as rare as leprosy is now. Even now, through the exertions of Koch, Cornet, and others, the German public is becoming aware of the need of disinfecting, as they recommend, all articles likely to be contaminated by phthisical patients, and already, according to Dr. Woodhead, there has been a diminution in the deaths from phthisis in the Grand Duchy of Baden from 3.08 in 1,000 in 1882 to 2.80 in 1,000 in 1887—a percentage of decline which Dr. Woodhead estimates would, in the British Isles, amount to a saving of nearly ten thousand lives per annum.

The second great fact is that, though tuberculosis is as communicable a disease as small-pox is, in that it never occurs except as it has been communicated from one sickened by it, yet, wholly unlike small-pox, it is not a contagious disease—that is, it is not communicated by mere proximity to a consumptive. It would be cruel, therefore, to treat consumptives as we must for our safety treat small-pox cases. Instead, experimental medicine tells us just what we are to do to arrest this infection—where we are to find it so as to destroy it after it has left the body of the consumptive—and if the medical science of our day had only done that it would still deserve the gratitude of all mankind.

We now come to the requisites for prosecuting the researches of modern experimental medicine. It will be seen that they imply the formation of a large body of investigators, operating on quite distinct lines of experi-

mental inquiry, and yet each branch of the service indispensable sooner or later to the others.

First in order is the worker in the physiological laboratory. Without his researches into the laws or mechanism of motion, sensation, respiration, circulation, secretion, nutrition, reproduction, and other living functions, many deductions of the other workers might be dubious till his verdict was rendered. For example, a very important question is now raised whether cancer is a parasitic disease like tuberculosis, because some epithelial cells in cancerous tumors are found with suspicious-looking cells inside of them. Are these latter parasitic invading cells, or do epithelial cells sometimes naturally—that is, physiologically—give birth to cells within them, or endogenously, as it is termed? It is obvious that that question must first be answered from the physiological laboratory ere the original question can go further.

Next comes the laboratory of experimental pathology. We may here quote the rather extreme views of Mr. Victor Horsley, in his address as president of the Section in Pathology of the British Medical Association at its meeting in 1892, of what relation experimental pathology has to pathology itself. He says: "However absurd the statement may appear to some, I venture to assert that pathology as such is almost unknown among us [in Britain]. The fact is that what is commonly spoken of as 'pathology,' taught as 'pathology,' and made the subject of examinations in 'pathology,' is nothing of the sort; it is not pathology; it is morbid anatomy. . . . We have in London a society whose function, as defined in its title, is to promote the study of pathology, but, unfortunately, it has hitherto been only an emporium of morbid anatomy. Thus, even when during the last two years as many as three hundred and twenty papers were communicated to the Pathological Society, not ten, and in 1890 not a single one, dealt primarily with the pathology of any disease. . . . The pathologist should be the student of disordered function as well as of disarranged structure; morbid

anatomy is therefore but one side of his work, pathology is the other. As, however, so far as I am informed, there are only three places in England where pathology is taught, the paucity of true pathological research among us cannot be regarded as a matter of surprise. It does not seem to be generally appreciated that what is required at the present day is the accurate determination of the observations of normal function, and, seeing what strides normal physiology as a science has made, no difficulty attends the pathologist in this new and fertile field."

Referring to a celebrated discussion on the origin of the dropsy in Bright's disease, which occupied several sittings of the London Pathological Society, and which was participated in by a number of the most distinguished physicians of that capital, he says: "Even in this very year, in a great discussion on the cause of dropsy, and one which appeared to many to be fruitful in little but surmises, opinions that dropsy is due to alterations in the blood pressure alone, and chiefly to venous stagnation, were freely promulgated and confidently relied upon. Yet they have for more than twenty years been absolutely contradicted by experimental pathologists and shown to be untenable. I have not the slightest hesitation in saying that this surprising ignorance of pathology is due to want of familiarity with modern progress in physiological and chemical research. Fortunately, laboratories are springing up now on all sides, original investigations are being pushed forward, and the reproach that we have been dead-house students rather than true pathologists will therefore soon be wiped away."

Mr. Horsley speaks rather warmly, being an experimental pathologist himself. His experiments on the artificial production of myxœdema in dogs by the extirpation of the thyroid gland led directly to the recent remarkable results in the treatment of that hitherto most serious disease in the human subject by the administration of thyroid extracts, thus opening an entirely new field in therapeutics, the full extent of which cannot at present

be estimated. But if, from various causes, there have been till lately in England but few laboratories for studying morbid processes in the same way that the physiologist investigates healthy processes, the reverse is the case in other European countries where such laboratories are attached to nearly every university and leading hospital.

Next in order comes the bacteriological laboratory. This generation will ever be known in medicine as the bacteriological era. The wonderful results in the saving of life by arresting the terrible scourge of puerperal fever, by enabling surgery to reduce the mortality from accidents or operations to a degree undreamed of twenty-five years ago, and the power which our profession now has to stop the march of the great Asiatic epidemic more effectively than any army has ever defended its country from an invader, have already familiarized the general public with the fact that a greater discovery has been made in this generation than the discovery of the uses of steam or of electricity. That discovery is the relation of the unseen earthly world of life to the seen. In actual bulk the living kingdom invisible to the naked eye is greater than the visible, because it is relatively omnipresent, while the visible is relatively local. But just as the visible vegetable and animal kingdoms are vitally interdependent, so this great discovery reveals the fact that, but for the invisible kingdom, the visible could not long continue to exist, for Nature soon would be choked with her own dead. It is to the activity of the world of micro-organisms that whatever has ceased to live is dissolved away into its original elements. The general relation of micro-organisms, therefore, to the other departments of life is beneficial and necessary. But it happens that there are some members of this vast host which can work mischief in animal bodies before death, each in its own way, and thus become directly causes of specific diseases. But how separately to identify any particular form among so many millions, and fasten upon it the indictment of a disease-producer, is a much more difficult

task than to single out one ruffian from the midst of a surging crowd of Gravesend voters and then prove the case against him. How deftly this has to be done in the case of bacteria, where so many look much alike, without having other bacteria confuse the verdict by crowding in along with the suspect, is illustrated by [any] one of the procedures recommended for getting a pure—that is, an unmixed—culture of the tubercle bacillus. As it is difficult to obtain pure cultures from tuberculous sputum, on account of the presence, just mentioned, of other bacteria, which grow much more rapidly and take full possession of the culture plate before the tubercle bacilli have had time to form their colonies, it is best first to inoculate a guinea-pig with the sputum from a consumptive, and to obtain cultures from it after the infection has become fully developed in it, which usually takes about three weeks. The guinea-pig should then be killed in a room wholly free from dust and its body laid on its back on a sterilized board, after its skin has been thoroughly washed in a 1:1000 solution of corrosive sublimate. Then, having sterilized beforehand a number of knives, scissors, and forceps by passing them through a flame, and quickly putting them under a glass bell jar till they are used, the operator, having already thoroughly sterilized his own hands, cuts the skin over the thorax and turns it back only just enough for his purpose. Fresh instruments are then taken to avoid pollution by stray germs, an opening is made into the chest cavity, the root of the lung drawn out, and a well-defined tubercle nodule is taken out with a slender, sterilized forceps and the nodule transferred to a sterilized glass dish. Time obliges me to leave the next details, how to inoculate the culture medium, except to say that finally, to keep the said medium from becoming too dry for the development of the bacillus, it is best to keep the cultures in a moist atmosphere, and to prevent evaporation by a rubber cap over the end of the test tube, which cap should be sterilized in a 1:1000 solution of sublimate for destroying

the spores of mould fungi, which in a dry atmosphere would be harmless, but under the rubber cap are likely to sprout and send their mycelium through the cotton plug to the interior of the tube, thus destroying the culture.

We have quoted all this merely to give some idea to those unfamiliar with the subject that the modern germ theory of disease is not built on theorizing, but, instead, on extremely careful work. Then it is but the beginning of this work to observe the forms or appearances of the microbes, though, to do *that* properly under the microscope, bacteria have to be stained with certain dyes, then washed in water or alcohol; then these dyes are in turn partially decolorized by chemical agents and then dyed again, each among a multitude of species often demanding a complicated formula of its own for the process. But, as we have said, after all this, further researches are needed to prove that this particular one is the sought-for kind, because other and very different bacteria may show the same appearance which it does. Thus, after Koch identified the comma bacillus of Asiatic cholera, it was soon alleged that a similar comma bacillus could be obtained from the dejecta of persons sick with ordinary cholera morbus; and then another comma bacillus, still more resembling Koch's, was reported as growing in old cheese. Pickpockets in a crowd may be suspected on account of their looks, but only their behavior justifies their arrest, and likewise it is necessary to observe the behavior of bacteria much more than their looks. This can be done only by finding suitable media outside the body in which they will grow. Growing microbes, however, is a very special business. In an ordinary garden it needs much industry to keep weeds out of particular beds, but how to keep microbe weeds out of a prepared culture medium often taxes the patience and skill of the bacteriologist beyond description. For a long time the whole investigation was retarded by difficulties of this kind when they were first cultivated in nutrient liquids,

and it is one of the great services of Koch that he devised the method of cultivating on solid nutrient surfaces, such as gelatin, agar jelly, etc.

With a final success in obtaining a pure, or unmixed, culture, the task begins of observing how they develop, what makes them grow freely, and what checks them, for only thus can be learned the most certain methods of destroying them—that is, of securing disinfection. But this entails more painstaking watchfulness than is demanded of any other cultivator, for often more is learned from the chemical effects of the growth upon its medium or soil than from any other results of the whole process. Hence it is easy to see that this branch of experimental medicine demands a precision in work which implies in the investigator not only a thorough training in microscopy and in chemistry, but also the possession of appliances and contrivances which make a modern bacteriological laboratory one of the most striking monuments of science which the world has ever seen.

Lastly, chemistry is indispensable to experimental medicine. This majestic science, whose field is conterminous with the material universe, in none of its branches is more important or more recondite than in the chemistry of life. It is now clearly recognized that, without chemistry, bacteriology would help us but imperfectly in the problems of infection. The earlier view, that bacteria work their mischiefs by themselves invading the blood or the tissues, and that they are to be discovered there, is now much modified by the knowledge that the action of many of the disease-producers is by their forming virulent chemical poisons which are absorbed into the circulation from local seats of bacterial growth. The microbes which do the deadly work of diphtheria do it all where they form the diphtheritic membrane, for as they grow there they generate three different chemical poisons, which are absorbed into the blood, and which have been isolated in chemical laboratories where such investigations are carried on, and which poisons injected into animals pro-

duce just the fatal paralyses, the fatty degeneration of the heart, and the general systemic changes which are found in human victims of this disease. The bacillus of typhoid fever and the micro-organism of Asiatic cholera both live in the intestine and there generate the poisons which, absorbed into the blood, give rise to the symptoms of these diseases. These and similar discoveries show that it is chiefly by their chemical products that bacteria produce their effects, and hence it is to chemistry that much of the whole problem of infectious disease will have to be referred.

But there is another great question now under investigation of extreme importance—namely, What is the reason that so many infectious diseases leave the patient immune against another attack? Two different answers are given—one from the laboratory of the chemist and one from the laboratory of the experimental pathologist. The chemist says that bacteria as they grow in a medium finally generate poisons which wholly neutralize their action, and that these secondary poisons can be isolated from the blood of animals which have become immune against the original poison of the bacterium, and that this antitoxine, as it is termed, can be used to arrest a case of the disease by its counteracting the disease poison itself. In the treatment of that terrible affection, tetanus or lockjaw, this has been finally demonstrated within the past year to be the case, and it is proposed to extend this principle to the treatment of pneumonia and of many other infections. On the other hand, many experimental pathologists maintain that invading bacteria are met by hosts of the white corpuscles of the blood, which crowd to the seat of the infection and forthwith attack the invaders by literally eating them up. It is then only when the invaders are too strong in numbers, or the white corpuscles have been weakened by some cause, that the battle is lost for the body. Probably both of these views—that is, the antitoxine and the phagocyte, as it is called—are right; but it is evident that these vitally important questions

are questions which only experimental medicine could either raise or settle.

There is still another wide field in medicine which falls to the chemist. The living body is constantly generating poisons of its own as it lives. To remove these is the business of the excreting glands. But it is now known that certain great organs have the conservative function of destroying or neutralizing some of these self-generated poisons. Thus it is maintained that poisons are generated every day of our lives in natural digestion in the intestine which, if they were not properly dealt with by the liver and other organs, would quickly kill us, chiefly by their poisonous action on the nervous system. We seem to have here a promising clue to the origin of many neuralgic, convulsive, or paralytic functional disorders, and therefore to indications for their treatment, to determine which we are to look to the progress of chemical dis covery.

Such being the different lines on which modern experimental medicine is pushing its investigations, the outlook for material gains in the next twenty-five years is fully justified by the splendid showing of special equipment for the purpose which the whole field of medicine now presents. Thus Italy, long ago the mother of medical schools for Europe, has recently, and almost suddenly, come to the very front again in her contributions to experimental medicine. One of the services of the young Italian school of investigators has been the following-up of the great discovery of the French surgeon Laveran, in Algiers, of the blood parasite which causes the ague disease, or malaria. They have good opportunities in Italy for such a study, and the work of Marchiafava, Celli, Camillo Golgi, and others in this field is fully confirmed by Sternberg, Councilman, and Osler in this country, by Prof. Vandyke Carter and Prof. Evans in India, and lately by the important work of Dr. P. Hehir, lecturer on pathology in the Nizam's medical school, Hyderabad, India. Here in our New York hospitals we

are also equally confirming the connection of ague with these blood parasites, the microscope thus aiding both our diagnosis and treatment.

The chief interest, however, of Laveran's discovery is not only in demonstrating the nature of one of the chiefest causes of what we call unhealthy places, countries, or climates, but that a new world is thus opened to us in the microscopic kingdom of life itself. For the malarial organism is not a bacterium, or vegetable parasite, but in its nature is an animal organism, or amœboid protozoon, as it is called. If that be so, it can scarcely be the only one of its kind. In fact, the discovery of disease-producing microscopic amœbæ is multiplying fast. Thus a peculiarly intractable and fatal form of dysentery is now demonstrated to be caused by a special amœba. But one of the most curious developments of the kind has been demonstrated in our own country as the cause of the Texas cattle plague. We may remark that the infectious diseases of animals are rightly regarded of as much significance as those of our own species. Thus the humble rabbit is now the object of the greatest interest and study to investigators in Naples, Berlin, Jena, Paris, Dorpat, London, and Glasgow, because it is infested by a very minute parasite, evidently not a bacterium, in the living cells of its liver ducts, and all this interest in the rabbit is because we may thus find out what cancer is. Now, in the case of the Texas cattle fever it is now demonstrated beyond question that it is caused by a protozoid parasite which enters and destroys the red blood corpuscles of the cattle, reducing them from seven million to the cubic millimetre to two million, and in the same manner that Laveran's protozoon enters our red blood corpuscles. But the further extraordinary fact has been demonstrated that the Texas protozoon gains entrance to the bodies of the cattle by means of a Texas species of cattle tick, for ticks which infest cattle in our Northern States never produce this fever. This death-dealing cattle protozoon, therefore, must live first on ticks as its host, and from the ticks

pass to the larger animal. Now, Livingstone first made the world acquainted with the formidable tsetse fly, whose bite destroys all bovine animals in certain localities in Africa by a slow wasting fever like the Texas cattle fever, and the mystery of its action—namely, that one such insignificant fly can kill an ox—may perhaps be found to be due to a similar infection to that of a Texas tick.

Thus this subject of animal protozoa is looming up in proportions which make us feel like the English naturalist Bates, when he had been working for eleven years in the vast Amazon forests, that the field seems to have neither beginning nor end. In fact, it seems to begin in our own blood in the case of our vast host of white corpuscles, which act very much like independent amœbæ or animals within us, whose business it is to eat up whatever should be eaten because it is growing old or because it has no business to be there. Hence they are called phagocytes, or eating cells, and Dr. Hehir thus describes how he watched a white corpuscle catch an actively moving malarial parasite: The phagocyte first advanced toward its victim, and then for about ten seconds appeared inactive barely touching its prey, and almost suggesting that it came to make friends with the unwary parasite, reminding one of the spider and the fly. That portion of the parasite in contact with the white corpuscle was apparently stationary, while the rest of it was making lively efforts to set itself free, but gradually its movements became imperceptible. The phagocyte continued to entangle its prey, until within the space of a minute it completely incorporated the amœboid parasite within its protoplasmic web, whence there was no exit.[1]

But to return to the Italian school of experimenters, who are largely disciples of instructors from beyond the Alps, such as the veterans Hugo Schiff, of Florence, and Moleschott, of Rome. Following up the discovery of

[1] Lancet, May 28th, 1892.

the tetanus bacillus by Nicolaier in 1884 in Prof. Flügge's laboratory at Göttingen, but which was first isolated in a pure culture by the celebrated Japanese bacteriologist Kitasato in 1889, the Italian investigators, Tizzoni and Cattani, developing Kitasato's suggestion, have succeeded in isolating an antitoxine to lockjaw from the blood of rabbits, which has been successful (in 1892) in eleven cases of severe tetanus in men, and with such promptness that no doubt of the curative efficacy of this remedy can be further entertained.

Crossing the Alps to the great medical centre of Vienna, we have a striking example of the advantages of associated medical investigation carried on in the same building. In the Vienna Pathological Institute we have first the chemical laboratory under Ernst Ludwig, to which is committed by the Imperial Board of Health all questions about the pollution of drinking water and like public questions. Then the famous pathological laboratory founded by Rokitansky, and the laboratory for experimental pathology under the superintendence of the veteran Stricker, while in another building is the physiological laboratory of Prof. Exner, well known for his work on cerebral localization, and the bacteriological laboratory under Weichselbaum, whose name is associated with the bacteriology of pneumonia. All these departments, liberally furnished by the Imperial Government with funds for the prosecution of original experimental research, co-operate together, each referring to the other any questions which may arise that call for a special report from its proper laboratory. In fact, Germany is the richest country in the world in laboratories of experimental medicine, with the result that she surpasses all other countries in her contributions to medical knowledge. Of the spirit of co-operation in this work and of its singular extent we need only cite, for illustration, the fact that the list of editors of the *Archiv für experimentelle Pathologie und Pharmakologie* contains the names of two professors of Leipzig, one in Munich, three

in Giessen, one in Halle, two in Prague, four in Königsberg, one in Zurich, two in Marburg, two in Berlin, one in Göttingen, one in Erlangen, one in Berne, one in Kiel, one in Greifswald, one in Dorpat, one in Frankfort, and three in Strassburg.

Such a list, however, only represents a fraction of the number of zealous experimental investigators in a country abounding with every kind of laboratory for every kind of medical research. There is, however, one aspect of this subject which we must very briefly refer to, suggested by an appeal which I have received, and which I suppose others present may have likewise received, from a society in London, apparently looking toward the formation of an international association for the suppression of all medical experimental laboratories, on the ground that they are little else than revolting establishments for cruelty to dumb animals. Now, it does not do to treat this serious subject with levity. It is no adequate answer merely to say, what is nevertheless the truth, that more animal suffering occurs daily at our slaughter-houses from which both vivisectionists and antivivisectionists get the meat for their tables, or to insinuate that the collation after the adjournment of an antivivisection society testifies to more barbarity in its lobster salad than ever happens in Koch's whole building, because the unhappy crustaceans therefor were carried alive from their cool marine beds to be soused alive in boiling water. The real question is, What is cruelty? If simply killing animals is cruelty, then every hunter for the sport of the thing is a monster thereof, and buying butcher meat is patronizing and sharing in cruelty. All Nature, then, is Satan's kingdom from the beginning, and no one else's. The line should be drawn just here. Inflicting suffering *per se* is not cruelty; only unnecessary suffering so inflicted is. This principle is impregnable, for no one can live in this world without inflicting suffering, and none but hypocrites can deny that statement about themselves. The question, then, is not that of inflicting suffering, but for what purpose is it inflicted?

That being so, we can strongly affirm that the intent of scientific men in experimenting on animals is so much the reverse of cruel that they may be trusted to devise the lessening of suffering in their work much more than ordinary butchers do in theirs; and as we allow these latter to kill in order that we may eat, we may let the men continue to work for the lessening of human sorrow and death, who already by that work have reduced, for example, the puerperal-fever mortality in the Vienna maternity from twenty-eight per cent to one-third of one per cent, who are saving now fully fifty thousand lives yearly in the field of surgery, and who bid fair to rid the world of all its epidemics as well as many of its hitherto greatest causes of suffering and death.

In illustration of this particular subject, and of our whole subject, we may give a short account of what a complete experimental medical institution is, as shown by the newly opened Koch Institute for Infectious Diseases in Berlin. Next to the new Infectious Diseases Hospital buildings, which have every arrangement for the isolation, treatment, and sanitation of the sick, is the Scientific Division building, a triangular structure, well lighted on all three sides. On the cellar floor on one side are the receptacles for the fodder or food for the stock of animals and the quarters for the animals themselves, whose cages are warmed by Kauffer's mantel ventilating heaters, which in winter force in a constant supply of fresh air. The rest of the space of this floor is used for an electro-motor, a water-distilling apparatus, a workshop, and for some of the heavier chemical apparatus. On the next floor are the rooms of the inspector and the assistant physicians who attend to the observations on the animals on whom experiments are being made, those already inoculated, etc., which are there in the compartments assigned to them, each cage having a card with the manner and time of treatment of its occupant. The next floor is used for bacteriological studies; at the apex of the triangle are the workrooms of Robert Koch, then those of his assist-

ants. These rooms are all furnished alike with a work table and its appurtenances at its centre, while at the large windows are the tables for working with the microscope, with every contrivance for lighting the microscope stage, with a digester, chemical reagents, and tools within reach. In each room there is a gas stove with its incubator for bacteria, having a thermometer so connected with electric wires at the two points of desired maximum and minimum temperatures that a bell is rung automatically whenever either heat point is reached. On this floor is the large incubating room, made on the model of that in Pasteur's Institute, being a room within an ordinary room, lit by electric light, and whose ceilings, walls, and floors are of different non-conducting layers of wood, brick, and clay, and the whole interior lined with lead to keep the heat confined. The entrance to the room is by means of a double set of doors so arranged that the outer door must be closed and locked before the second door can be opened at all. On the walls are the leaden brackets for holding the cultures of bacteria. The heating of this room by hot-water pipes is automatically regulated by electric wires and bells, so that the temperature never goes below 37° C. or above 40° C. On the next floor is the chemical laboratory, with rooms for the chief of that department and of his assistants, with all their requisites for work; also the department for microphotography, with its full apparatus, the dark room of this laboratory having its floor cemented and varnished for further insuring against the defilement of dust. Lastly, on the next floor comes the library, that most indispensable adjunct of experimental medicine in these days of incessant publication of researches from Japan to America, and where we may be sure there are no dusty volumes.

If the ancient Greeks, among their gods for every quality, had a god for accuracy, they certainly would have erected his image over the portal of such a temple as this. Everything here is devised under the inspiration of that quality. The attending priests have to go through ablu-

tions and washings of utensils which would have worn out the most scrupulous observer of the rules in Leviticus. It is curious, indeed, to read in the old Mosaic injunctions that leprosy might so infect the walls and plastering of a house that the priests had the right to condemn it and order the house to be taken down (Leviticus xiv. 33–49). The modern discovery, therefore, that the bacillus of leprosy closely resembles the bacillus of tuberculosis might be commended to some modern critics as another proof that the old Egyptian priest Moses could not have written the Pentateuch, but that it is a very recent book indeed.

Time forbids us to more than mention that great centre of experimental medicine, the University of Dorpat, in the German Baltic provinces of Russia. For years the reports of its physiological laboratory under A. Schmidt and of its chemical laboratory under the veteran Dragendorff have been widely known in the world of medicine; and of their younger colleagues, Prof. Thoma stands in the front rank of bacteriologists, while the recently published work of Prof. Kobert on the action of drugs as interpreted by experiments is generally received as the most authoritative work in its department.[1] In St. Petersburg itself the new Imperial Institute of Experimental Medicine, erected by the government and by the munificence of Prince Alexander Petrovich, who is its curator, is the largest establishment of the kind in the world. It includes no less than eight large buildings, and nothing has been spared to make it equal to Koch's Institute in Berlin and the Pasteur Institute in Paris. With the efforts made to fill its chairs with the most eminent professors who can be secured, there can be little doubt that it will soon be another of the great centres of scientific activity. In Sweden the ancient University of Upsala is known to all physiological chemists by the work of Prof. O. Hammersten. In Norway, Prof. Lochman's labora-

[1] To the abiding disgrace of Russia, this great university has been suppressed this year because it was German.

tory at the University of Christiania is one of the resorts of English workers in original research, who till recently were denied such opportunities for prosecuting these investigations in their own country.

France is second only to Germany in the field of experimental medicine. Thus the English student of bacteriology has to learn the two languages, French and German, if he is to keep up with the subject at all. It is to France, indeed, that we owe the real beginning of our knowledge of the true relations of the invisible earthly world of life. Never in the history of human knowledge has it been given to one man to make a greater addition to it than that contributed by Pasteur. One cannot help feeling a glow in reading the words addressed to him on his salutatory, which describe how, beginning with his researches on fermentation with the lactic-acid-making yeast, he went on to the agents which produce putrefaction of animal substances, which he showed was a strictly analogous process with fermentation, till he came to this memorable conclusion: that "whenever and wherever there is decomposition of organic matter, whether it be the case of an herb or an oak, of a worm or a whale, the work is exclusively done by infinitely small organisms. They are almost the only agents of universal hygiene, as they quickly clear away the remains of all that has had life. They protect the living against the dead. They do more. If there are still living beings, if since the hundreds of centuries the world has been inhabited life continues, it is to them we owe it."

It is the Pasteur Institute in Paris that has been the model for all other institutions of experimental medicine, not only in France, but also in all the world. The impulse to prosecute experimental researches has extended to every large hospital and medical school in the country, and the published researches of Gamaléia, Telamon, Roux, Yersin, Chauveau, Koga, Bouchard, Gautier, with the naturalized biologists, Metschnikoff and Soudakewitch, and a host of others in every department, show how well

France maintains her old place and rank in the world of practical science.

The profession in Great Britain has been long fettered by legislation hostile to all forms of experimental medicine which involved experiments on animals. It is a curious chapter in history, with many of the features of a religious crusade. But now that the British public is learning better what sort of beings physicians are and what their aims are—namely, to spoil their own business as much as possible by *preventing* disease—English investigators are no longer obliged to go to foreign countries, but can work at home in London at the Conjoint Research Laboratories of the Royal College of Physicians and the College of Surgeons, at the bacteriological laboratory of the College of State Medicine, and at the Brown Institution, where experimental physiology and pathology are carried on. Besides these, there are physiological and pathological laboratories at University College, King's College, Guy's, St. Thomas', St. Mary's, and Charing Cross Hospitals. At Cambridge there are the physiological and pathological laboratories of the university, and also at Owens College, Manchester; at Oxford, a physiological laboratory only. The University of Edinburgh is well supplied with laboratories in physiology, pathology, bacteriology, and sanitary science, and the Royal College of Physicians of Edinburgh has established its research laboratory. In Ireland, Queen's College, Belfast, has both physiological and pathological laboratories. This new movement and the former impetus from the Continental schools have already borne good fruition. We may simply mention in illustration Gaskell's researches on the contracting, expanding, and conserving rôle of accelerating and of inhibiting nerves; McKendrick's on the gases of the blood; Macalister on the physiology of heat production; William Hunter on pernicious anæmia and on the physiology of the destruction of blood corpuscles; Sidney Martin on the blood poisons of diphtheria; and the important contributions to the physiology of the nervous system by

Ferrier, Horsley, Beevor, Schaeffer, and others. The younger school of bacteriologists is also rapidly assuming its proper status, as was shown in the important debates last year at the London Pathological Society and at the British Association on the subject of immunity. Lastly, to the illustrious Sir Joseph Lister the whole world is indebted for his early appreciation of the bearing of the immortal discoveries of Pasteur on surgery, and his triumphant demonstration of their practical application in the new surgery which will ever make the nineteenth century memorable in the annals of medicine.

In the field of experimental medicine our own America has shown hitherto that she is a new country in more than one sense. It would seem to be the fashion with many to praise whatever is new and depreciate whatever is old on principle. But we might as well prefer new wine to old as to welcome perfect newness in education, literature, or science. There was much that was crude enough in the medical education of the United States of twenty-five years ago. To get an adequate medical training the tide of emigration had to be reversed and Americans had to go east. A better era, however, is now fairly begun. We have three laboratories in this city alone in which medical research is and can be carried on in either one of them with much better facilities than could be found in all America twenty years ago. But the laboratories of the College of Physicians and Surgeons, the Loomis Laboratory of the University, and the Carnegie Laboratory of the Bellevue Hospital College are things of yesterday, and yet already the work of members of this Academy therefrom is now quoted and appealed to in the literature of the world's scientific brotherhood—a rare occurrence in former times. Meantime Johns Hopkins University has attained a foremost rank among the institutions of the world for original research, while the laboratories of Harvard, Yale, and Pennsylvania Universities, and of the University of Michigan, are each new centres of medical investigation, which bid fair to represent our country as

she should be represented in the field of modern science. Our medical colleges, however, have been at a great disadvantage in comparison with similar European institutions, in that they have been so largely private institutions, with no support or recognition from the Government or even from the general public; and when they have been associated in name with academic colleges the connection has often been anything but integral or organic, more as if they corresponded to an appendix vermiformis, which might be easily cut off without being either missed or regretted. It has been therefore mainly by private munificence, but lately given in any case, that America has been able to take a position in experimental medicine at all, and yet we can already point to such names as Welch, Councilman, Sternberg, Prudden, Byron, Wood, Chittenden, Vaughan, and others as men for us to be proud of.

Rather unexpectedly to some, the municipal government of the city of New York is to be credited with one good thing, and that not a small one. I bespeak here the full appreciation and co-operation of the Academy and of the profession of our city in the work of the Health Board, under the supervision of Dr. H. M. Biggs, for the early diagnosis of diphtheria by bacteriological methods, for which he has devised a simple and effective system which enables any practitioner to send an inoculation of his own from the throat of a suspected case to the Health Board bacteriological laboratory, and to have the diagnosis settled within twelve hours. The practical value of this system is great, for every physician knows that we ought to have no doubt about a throat exudation in any case; for true diphtheria is always dangerous, not to the patient alone, but to everybody, and yet in many instances only a bacterial cultivation will give certainty to the diagnosis. Moreover, diphtheria does not always disappear with the membrane, for enough of it may remain to both inoculate a culture plate and to infect others, when only naso-pharyngeal mucus is present to inspection, and such children may be allowed to play with others or go to school,

with the most fatal results. Dr. Biggs has charts which by the graphic method conclusively demonstrate how single cases of diphtheria have started neighborhood epidemics in our city, just as a fire caught in a room spreads first to other rooms and then to surrounding houses; and this valuable work should lead to dealing with this fell disease by resorting to the same measures for removing and isolating the sick, till all its germs are known to be gone, that the community now allows to be done by law with small-pox.

In conclusion, we protest against a word of what we have said about experimental medicine being construed as depreciating either clinical medicine or pathological anatomy. Showing the fine fruit on one branch of a tree certainly detracts nothing from the excellence of the fruit on the other branches. More than that, experimental medicine never can be a *substitute* for clinical medicine. We might as well fancy that microscopes are going to be so wonderfully improved that they will not need eyes to see through them. Experimental medicine is but a new handmaid to clinical medicine, coming now just at the right time to aid us at the bedside toward clearer diagnosis, clearer appreciation of conditions, clearer prognosis, and last, but not least, to less empirical and to more scientific treatment.

Secondly, it is time that the general public should appreciate how universally beneficial all progress in medicine is to the rich and to the poor alike. Medical science knows absolutely no distinctions of circumstance, age, sex, culture, or brain. All fall sick together, and all have the same claim to be kept from falling sick, or to be helped if they do. Is it too much, then, to hope that in the coming century a small fraction—for that is all that we would think of asking—of the public money now spent on ironclads, for example, with apparently the result of showing how well they can sink, will be given to our workers to enable them to rid the world, say, of malaria? Why, even real estate men might subscribe toward such

an object, as a mere matter of land speculation. Seriously, our generous donors, following the example of that practically minded man, William H. Vanderbilt, should consider whether they cannot as wisely contribute of their wealth to institutions which will fit men to make accidents to limb and body less formidable, or who will smite pestilence by day or by night, or who will lessen bodily suffering in too many ways to recount, as to go on endowing colleges for undergraduate boys.

Lastly, I take my leave, with many apologies for such a long discourse, by saying that this review emphasizes how truly international the brotherhood of medicine is. Fancy an attempt nowadays to have the lawmakers of the world co-operating together, legislators in England comparing notes with the councillors of the czar, and receiving suggestions from each other! But harder yet is it to conceive of co-operation between Rome, St. Petersburg, Tokio, Edinburgh, Boston, and all other places between, in questions religious! But in medicine we do not even know about one school of doctrine or another. From the nature of the case ours is a world-wide, zealous partnership, all working together on equal terms for one end, and inevitably that end will come to pass in a world made by medical men much the happier because much the healthier.

Stated Meeting, December 7th, 1893.[1]

INTESTINAL APPROXIMATION. PATHOLOGICAL HISTOLOGY OF REUNION AND STATISTICAL ANALYSIS.

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Mr. President, Fellows of the Academy: In accepting the invitation of your worthy chairman, Dr. Bryant, to read a paper before this great scientific body, I appreciate the honor conferred upon me, and realize the fact that in this audience will be gathered many men who have spent the better part of their professional life laboring in this special field, and whose achievements are an ornament to surgery, a heritage to the profession, and of incalculable value to humanity.

I am greatly relieved by the knowledge that I will be assisted in the presentation of the various aspects of the subject by the gentlemen who are to participate in the discussion. If some phases of the subject of intestinal approximation appear to be treated cursorily, it is because the gentlemen who follow have had a greater experience in certain procedures and are therefore better able to present the subject from a practical standpoint. I realize that I have no novel propositions to make, nor have I a new device or method to present for your consideration. It is my desire to discuss the pathological histology of reunion, and to give an impartial retrospection of the clinical achievements by the different methods under the same favorable circumstances. Therefore I will not enter into antiquities, nor even into the mediæval

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history of intestinal anastomosis, but will confine myself to the records of the last five years. The reason for this is apparent, as no method of approximation, however perfect in the pre-antiseptic period, could give the favorable results which are attainable at the present day. With a slight reservation the same may be said of the methods in the antiseptic epoch. In the first period I include all before the advent of Listerism, in the second the methods employed from 1872 to 1888. In this period the knowledge of physiology and histology upon which the technique of intestinal surgery was based, as well as the technique itself, was imperfect, which was very detrimental to the success of operative procedures. In the third epoch, from 1888 to the present time, all methods have had equal opportunities to produce the same results, as the principles governing intestinal anastomosis have been well established.

In order to attain good results in intestinal approximation we must consider the means of—

1. Securing accurate contact of surfaces.
2. Producing speedy and permanent adhesion of the approximated portions.
3. Maintaining an opening sufficiently large for immediate purposes.
4. Producing a cicatrix that will not contract to a deleterious degree.
5. Accomplishing all of these in the most *simple* and *rapid* manner.

The means that have been employed to attain these ends are well classified as follows:

1. Suture.
2. Suture with mechanical aid.
3. Mechanical means.

Before considering the special application of the various methods, let us devote a few minutes to the anatomy of the organs to be approximated. For purposes of approximation the points which concern the surgeon most are:

1. The strength of the wall of the viscus.

2. Its histological power of agglutination and regeneration.

I have determined experimentally the strength of the walls of the various hollow viscera of the digestive tract against diastaltic pressure, with the following results: The wall of the stomach in large living adult dogs withstood an average pressure of eight pounds before the peritoneum ruptured; it required nine pounds to rupture the entire wall of the stomach. The peritoneal coat of the duodenum gave way opposite the mesentery under a pressure of ten pounds. The lower portion of the ileum withstood a pressure of nine pounds when it ruptured on the convex side; the peritoneal and muscular coats gave way under six and one-half pounds. The ileum, two yards above the ileo-cæcal valve, ruptured under seven pounds. In the middle portion of the jejunum the peritoneal coat ruptured at seven pounds, and the entire wall at eleven pounds on the convex side. The lower four inches of ileum, cæcum, and four inches of colon ruptured at seven and one-half pounds for the peritoneal coat and at eight and one-half pounds for complete rupture. The rupture invariably occurred on the convex side of the ileum, one-half to three-fourths of an inch from the attachment to the colon. The colon ruptured at fifteen pounds, the muscular, serous, and mucous coats giving way before the tunica propria.

With a tube inserted in the papilla of the ductus communis choledochus, the pancreatic duct withstood eleven pounds pressure before rupture. The gall bladder and cystic duct resisted a pressure of thirteen pounds before the gall bladder ruptured.

In only one of the experiments on the alimentary canal, including the gall bladder, did air escape into the mesentery, showing that these hollow viscera in the dog possess a greater resistance at the seat of the mesenteric attachment, where there is no peritoneum, than in the portion covered by peritoneum. It was noted that under three to four and one-half pounds pressure small bubbles of air

could be seen passing through the mesenteric veins. The order of rupture of the coats was: (1) peritoneal and muscular coats, in an irregular line, in the long axis of the bowel; (2) mucous coat, which was torn in shreds, transversely to the axis of the bowel; and finally the tunica propria, which always ruptured parallel to the long axis of the bowel. When the mucous and submucous coats were divided before the distention, the other coats ruptured under one and one-half pounds pressure.

The following were the average results obtained on cadavers four days after death: Peritoneal and muscular coats of stomach lacerated extensively under three and one-half pounds pressure, and ruptured always on the anterior surface of the lesser curvature, close to the mesenteric attachment, under four and three-quarter pounds pressure. Jejunum ruptured under a pressure of four and one-half pounds, always on the mesenteric side, the peritoneal and muscular coats on the convex side giving way before complete rupture. The colon to within four inches of the ileo-cæcal valve ruptured under four and one-half pounds pressure. The lower four inches of the ileum, cæcum, and eight inches of the ascending colon included in the same ligatures, ruptured under a pressure of seven and one-half pounds, the rupture occurring on the mesenteric side of the colon. The peritoneal coat of the sigmoid flexure ruptured under a pressure of two and three-quarter pounds, and the tunica propria under four and three-quarter pounds. With the tube inserted in the papilla of the ductus communis choledochus, rupture of the pancreatic duct occurred under a pressure of five and three-quarter pounds; with the tube inserted in the cystic duct, the gall bladder ruptured on the peritoneal side under a pressure of twelve to twenty-two pounds.

These experiments show that the resistance of the wall of the gastro-intestinal tract against diastaltic pressure is sustained (1) by the mucous, muscular, and serous coverings, and (2) after a rupture of these by the submucous coat (tunica propria). The mucous membrane offered

but very little support to the wall of the bowel, as it tore into transverse shreds over the entire circumference during the process of distention (see Fig. 1).

To maintain the strength of the bowel it is necessary to have the submucous and serous coats of the wall united

Rupture of mucus membrane of bowel Fig. 1.

at the seat of approximation. The tunica propria is used as a mechanical support, but its regenerative power is slow and very limited, while the peritoneal coat has great and rapid power of adhesion and proliferation. Therefore the serous coat is the one upon which the surgeon

Rupture of serous coat of bowel. Fig. 2.

must depend most for primary agglutination and adhesion on account of these two properties of the peritoneum, and it must always receive the greatest attention in all methods for producing intestinal anastomosis. It has been repeatedly observed that moderately firm adhe-

sions between two approximated, abraded, or injured peritoneal surfaces form in six hours when the surfaces are aseptic.

After isolation of the layers, the mucous membrane, muscular layer, and serous coat tear in all directions with equal and moderate force. The submucosa cannot be torn transversely, but with great force it ruptures. It can be torn longitudinally with considerably less pressure. We must depend upon the submucosa for retention of the coaptation. The Lembert suture should therefore be inserted, when we desire to give the union its greatest strength, transversely to the longitudinal axis of the bowel, and should embrace the submucosa, as by Cushing's right-angle suture. If the surfaces are septic, union will not take place, but should they temporarily adhere the exudate will rapidly disintegrate and the surfaces separate. Asepsis is therefore a factor of great moment in securing permanent union after intestinal approximation. No matter what means are employed to produce the approximation, the surfaces will not unite if they are septic, for the same reason that we have an absence of primary union in septic wounds in other parts of the body.

In reporting autopsies following non-union in intestinal approximations, it is important to state whether a septic peritonitis existed at the time of operation.

Histology.—From the articles on the pathological histology of the union of the bowel with Czerny-Lembert suture by Ritschl and Walter Rindfleisch, we take the following: It is important to know from many standpoints how the reunion of a divided intestine takes place, how rapid the adhesion, how soon definitive union occurs, and what disposition is made of the suture material. Up to the present time the greatest attention has been paid to the pathology of perforations, the causes of death following, and the new methods of approximation, and little attention has been given to the pathology of the reparative process itself. An examination of an end-to-end ap-

proximation of the intestine (Czerny-Lembert suture) forty-eight hours after the operation shows the mucous membrane very much contracted and an exposed free submucosa. The submucosa is covered with lymph and pus corpuscles; the seat of approximation is recognized by a striated, infiltrated tissue. The muscular coat has undergone no perceptible change. The serosa and omentum are notably thickened and congested. The stitch holes can be observed, the surrounding tissue is densely infiltrated, and an intensely dark ring is around the stitch canal. Coils of intestine are adherent to each other at the site of suture and bent at short curves.

Fourth day. Firm adhesion; suture does not appear on surface; around many of the sutures stitch abscesses have formed. Knuckles of intestine firmly adherent to wall at seat of suture, bent at sharp curves, but still admit the passage of water.

Microscopically the union of the ends is not produced by a juxtaposition of separated coats of the incised surfaces, but the connection is made by a zone of inflammatory infiltration with a substratum of the remains of the serosa and muscularis. The mucous membrane has healed over the formerly exposed submucosa; its base protrudes and rests upon the cicatricial tissue, so that many of its glands are deposited thereon. In the mucous membrane, especially in the tunica propria, there exists, for three or four centimetres, a catarrhal condition not infrequently accompanied by wandering leucocytes above and below the union. The submucosa is almost approximated from either side, and but very little changed from its normal condition. A very slight cell proliferation and enlargement of the vessels exists, so small that it can scarcely be distinguished from its normal condition. The muscular coat shows a marked cell proliferation and vascularization, and takes part in the formation of the scar. The border of the serosa is obliterated in its union with the omentum, and is thickened at the seat of the suture from five to ten times its normal size. One stitch abscess is

formed where the stitch drew in a portion of the mucous membrane.

Eighth day. At the seat of suture, coils of intestine are adherent to the sutured surface and also to each other, forming a bunch. The descending portions of the intestine are grouped around the site of the suture in short curves. The adhesions are broad and firm; they cannot be torn, but must be cut with scissors. Notwithstanding the short curves, there is no angle produced sufficient to obstruct, the lumen being free throughout.

The approximation projects above the level of the surface of the mucous membrane, and there is an ectropion of the mucosa from the scar.

Microscopical Examination.—At the muscularis the tissue is retracted; there is a disarrangement of the direction of the fibres, with a small multiplication and infiltration of cells recognizable. The serous surfaces are united to each other and to the omentum without much change of their structure, although there is a thickening of the zone by inflammatory exudate.

Twenty-first day. Omentum adherent over one-third of line of suture. The bowel contracted at the seat of suture to the size of a dime, while above and below the intestinal lumina were equal and about the size of a nickel. On the mucous side is found a small furrow with sharp edges, showing the seat of cicatrization. There is a small, light-gray scar on the furrow.

Microscopical Examination.—The mucous membrane is united without interruption. The muscularis mucosa is separated by a distance of from one to two millimetres. The greatest irregularity prevails in the union of the muscularis, the muscular fibres being distributed in all directions. The serosa is somewhat thickened. The silk can be seen unchanged, and surrounding it is a cell infiltration. The tunica propria does not differ in the least from its normal condition. There is a spindle-shaped termination of the muscular coat in the scar. The new formation of muscle cell is inconsiderable.

Eighty-sixth day. Omentum adherent at site of suture. The intestines adherent in very short curves, but no angle produced. No obstruction. There exists a circular constriction; the lumen at the seat of suture is about the size of a nickel. This circular contraction is formed by an elevation two or three millimetres in thickness above the surface of the mucous membrane, which is always produced by a Czerny-Lembert suture.

Microscopical Examination.—There is an elevation upon the mucous surface, in the centre of which is a depression where the mucous membrane rests upon the submucosa. The submucosa is unchanged except on its extreme outer limits, where its fasciculi extend into those of the cicatricial tissue. The muscular wall is not perceptibly changed. The serosa in the scar has lost its identity entirely, while on the surface it is united to the omentum, passes continuously over the scar, and has not changed its structure. The suture material is unchanged; it is surrounded by an area of infiltrated tissue of numerous large cells with large nuclei.

One hundred and sixth day. Omentum adherent. The calibre at the line of suture, compared with the intestine above and below, is relatively as a dime is to a nickel.

Microscopical Examination.—The mucous membrane shows no evidence of a previous separation. The muscular layers are connected by fibrous bands through the scar tissue. The muscular tissue up to the line of approximation is absolutely unchanged. The serosa gradually thickens as it approximates the scar, is very vascular, and its tissue becomes more dense, which is caused in particular by an increase in the number of spindle-celled elements. Silk unchanged.

One hundred and thirtieth day. The intestine above and below the scar is of equal calibre.

Microscopical Examination.—The mucous membrane united perfectly, showing no evidence of line of union. The circular muscular fibres held together by a thin band of connective tissue. The longitudinal muscular fibres

are much more firmly united, the fibres from one side to the other being continuous.

Conclusions.—From the above description of the pathological conditions the following deductions may be drawn: The ectropion of the mucous membrane after an incision is passive and is due to the elastic retraction of the underlying submucosa and muscularis. For this reason, by the Czerny-Lembert suture, there is an almost uniform coaptation of the edges of the mucous membrane without the aid of a special suture, which protects by its intimate approximation the underlying tissue from exposure and infection. This would not occur with a Lembert suture alone, as there would be an overlapping or separation without the Czerny. When there is a defect in coaptation of the mucous layer, union must take place by secondary intention and the tissue is exposed to all the dangers of infection—viz., non-union, fistula, and circumscribed abscess—the same as in secondary union upon the cutaneous surface. Glandular tissue is not regenerated over this area.

The Submucosa plays an important rôle in intestinal approximation from the moment of coaptation. While it takes but very little part in the regenerative process, we depend principally upon it for support given the suture in retaining the bowel in position. The more transversely the stitch is inserted the greater its support. Later, a short distance back of the line of suture, there is a pronounced vascularization of the submucosa.

Muscularis.—When an exudate intervenes between the opposing muscular layers the muscle fibres terminate in spindle-shaped processes. When the apposition is perfect the fibres from the opposing sides unite so accurately that the line of union cannot be discerned.

Serosa.—The serosa plays the important rôle in the inflammatory or regenerative reaction. By it we have produced the first adhesive exudate, which process commences immediately after approximation, and in a few hours cements the recently united surface, as supported

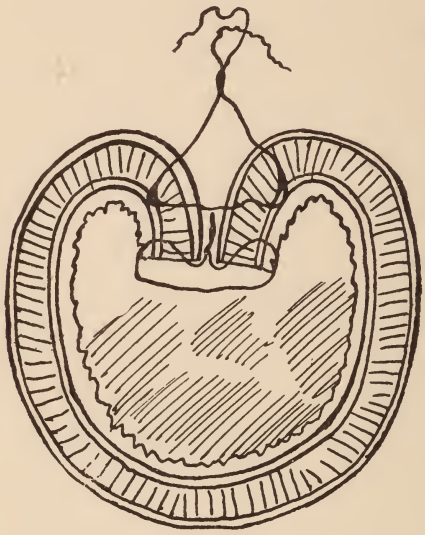
by Jobert's principles. The vascularization and thickening of the serous coat commences and reaches about one-third centimetre in thickness three to four centimetres away from the line of union, and becomes more dense as it approaches the line of suture. The omentum that becomes adherent is also greatly thickened by cell infiltration and increased vascularization, and on the eighth day the adhesions are so firm that they cannot be separated without lacerating the subjacent tissues. Frequently the neighboring coils of intestine become adherent at the line of union; the great danger of this is that the loop may be so short as to produce an acute angle in the coil and thereby cause obstruction. The resistance of the bowel against diastaltic pressure is offered almost entirely by the submucosa and muscular layers; they resume their normal functions after a perfect union. From this it will be seen that, in order to procure a perfect result in intestinal approximation, a primary union, not necessarily of all the coats, but of some, is imperative. It is not necessary that they should adhere to each other, but that the various coats should approximate as closely as possible. In the great majority of cases following suture of the bowel there are adhesions of coils of the intestine, mesentery, and omentum; as a rule they do no harm and, I believe, aid in retaining the bowel in the proper direction at the site of union, as well as supporting and protecting the line of suture. There occurs of necessity with the Czerny-Lembert suture a degree of stenosis; this is produced by a protrusion of the Czerny portion of the approximation into the lumen of the bowel through the approximation of the serosa with a Lembert suture (see Fig. 3). F. Byron Robinson found that this produced a complete occlusion of the bowel in three weeks in the dog.

The following is a report, furnished me by Dr. E. H. Lee, of pathological changes after the use of the button; of a specimen four days after operation, the button had been voided twelve hours previously, the shortest time in which a button had liberated itself.

Omentum firmly adherent one-half inch above and below the line of union. Thickness of the wall of the bowel the same at cicatrix as on either side.

Microscopical appearance of transverse sections of cicatrix:

1. Serosa thickened about seven times its normal size by infiltration within and exudation upon its surface.



Czerny-Lembert Suture - Fig 3.

The line of adhesion between it and omentum cannot be discerned.

2. The longitudinal muscular coat is not united at seat of approximation, though the ends stand in direct apposition to each other.

3. The circular muscular coat is in the same relation and shows granulations.

4. The mucosa is entirely separated over cicatrix; the edges atrophied and overlap underlying layer.

5. Regeneration of tissues across the line of union has not yet commenced, except in peritoneum.

Prof. Ludvig Hektoen gave the following report:

Longitudinal section through line of union in the small intestine of a dog thirty days after end-to-end approximation with button. I. *The Serosa.*—This layer appears but very slightly thickened; it is closely applied to the longitudinal muscular coat over the precise line of union, and beyond this there is some subserous areolar tissue in which are a few blood vessels as well as lymphatics.

II. *The Longitudinal Muscular Coat.*—Quite large parts of this coat are continuous across the union line, but there are masses and bands of cicatricial tissue distributed between these different continuous muscle layers. On each side of the line of union the muscular tissue of this coat appears absolutely normal.

III. *The Circular Muscular Coat.*—This part of the wall is completely interrupted along the line of union by interlacing narrow bands of fibrous tissue that enclose small masses of transversely-cut muscle fibres and extend to the mucous coat. Immediately to one side of this point the circular coat is replaced by a district of loose areolar tissue containing large blood vessels with very thick walls and irregular quantities of transversely-divided muscle fibres; beyond this district the muscular layer appears normal. To the other side of the union line appears a mass of transversely-divided muscle traversed by bands of fibrous tissue, and then comes an area of loosely-meshed tissue containing very thick blood vessels, and next the normal muscular layer.

IV. *The Submucous Coat.*—As already stated, interlacing bands of fibrous tissue extend through this layer from the muscular coat. On each side of the line of union the submucous coat appears normal, except that on one side the tissue seems rather dense. There are no lymphatic structures in this part of the intestine.

V. *The Mucous Membrane.*—The muscularis mucosæ is

completely interrupted, the glands resting upon the cicatricial tissue traced upward from the serosa on a line even with the ends of the muscularis mucosæ, which appears otherwise normal. The villi are largely absent, not only over the line of union, but elsewhere, and this is most likely due to the rough and extensive handling the specimen has met with since its removal. The mucosa is continuous across the line of union, but much thinner here than elsewhere, and made up of somewhat irregularly arranged glandular tubules lined with short columnar cells; some of these tubules are cut transversely, others longitudinally or obliquely; between the tubules is some small-celled infiltration, and projecting from the free surface are irregularly shaped, large and small villous projections without the usual columnar-celled lining. On each side of the line of union the mucosa appears quite normal, with perhaps a few more small round cells between the tubules than usual.

Recapitulation.—The line of union, which is almost linear in thickness, can be traced by means of the cicatricial tissue, which, though very small in amount, is distinct. The various layers appear to be in perfect juxtaposition. The longitudinal muscular coat can be said to be almost restored; the transverse muscular, submucous, and the muscularis mucosæ are completely united by connective tissue at the line of union, while the mucosa proper has undergone partial regeneration.

Longitudinal section through line of union in small intestine sixty days after end-to-end approximation by same means.—In this specimen the restoration of the various layers in the intestinal wall is much further advanced than in thirty days' specimen; in some respects the restorative changes are complete. The various layers have been held in complete juxtaposition and are continuous. There is no thickening of the serous coat except just across the line of union. The longitudinal and circular muscular layers are continuous and not distinguishable from those in the normal intestine, except for this, that they contain some

small spaces (blood vessels) and occasional traces of fibrous tissue. The greatest change is seen in the submucosa, which has been converted into thick cicatricial tissue containing vessels with very thick walls. Corresponding to this point of submucous cicatricial thickening, the muscularis mucosæ shows considerable increase in thickness also, due largely to fibrous tissue between the muscle layers which are now continuous. The mucosa proper over this district does not in any way differ from the mucosa elsewhere, and were it not for the thickening described in the submucosa and the muscularis mucosæ it would be difficult, if not impossible, to recognize the line of union.

I believe the pathologists have furnished the solution of the slight contraction following this method of union, viz.: (1) the juxtaposition of coats—*i.e.*, apposition of homogeneous histological elements, with re-establishment of their continuity; (2) the very small amount of connective tissue intervening between the ends of tissue that regenerate slowly if at all.

In making a comparison of the results obtained by the various methods employed in producing intestinal approximation, there are certain conditions which cannot be accurately estimated, and which must always cause an element of doubt as to the reliability of the deductions drawn from statistics alone. The *first* of these conditions is the frequency with which unfavorable cases are omitted from the reports, contrasted with favorable ones; *second*, the condition of the patient at the time of operation, as some surgeons will undertake a serious operation when the patient is moribund, while others will operate only under favorable circumstances; *third*, the anatomico-pathological conditions for which the operation is performed; *fourth*, the ease with which a means of producing approximation can be applied increases the frequency of its use, as it is resorted to in desperate cases where more complicated measures would not be attempted, thereby increasing the chances for a greater percentage of un-

favorable results; and *fifth*, the dexterity of the operator. A striking example of the latter is shown in the results obtained by Riedel in his operation of cholecystostomy in two sittings. He performed this operation thirty-four times with thirty-four consecutive recoveries; while in twenty-five operations of the same kind performed by all other operators there were six deaths, a mortality of nearly twenty-five per cent. Another example is the rapidity with which Abbe can insert thirty-four inches of continuous suturing in his lateral approximation operation, and the good results obtained—five recoveries in six cases.

With these facts in view we will proceed with our analysis of the results, beginning with gastro-enterostomy.

Gastro-enterostomy.—The pathological lesions for which this operation is performed are divided into two classes: (1) neoplasms involving the pylorus or duodenum, producing obstruction, and (2) cicatricial contractions occluding the pylorus or duodenum. The most common cause of obstruction is carcinoma of the pylorus. In some cases of this disease the obstruction is the first marked symptom of its presence, while in others it does not appear until the disease is very far advanced and the patient greatly emaciated and cachectic. The disease which produces the obstruction in the first class of cases, without excision, necessarily terminates fatally, and the operation of gastro-enterostomy in these cases is only for temporary relief. The patient should not be operated upon unless there is a reasonable chance of considerably prolonging life. While the patients may withstand the immediate effects of the operation, after four or five days they become weaker and weaker, until they finally succumb to the general depression, to which has been added the depressing effect of the operation. The number of cases of this variety operated on by various methods, and the results, are as follows:

GASTRO-ENTEROSTOMY FOR MALIGNANT DISEASE.

	Cases.	Recoveies.	Death.	Percentage of mortality.
Total number of operations	85	53	32	37.6
Method: by suture	28	16	12	42.8
Mechanical means	49	36	13	26.5
Suture with mechanical aid	8	1	7	87.5
Of these the position was:				
Side-to-side	76	47	29	38.2
End-to-side	4	3	1	25.
Unknown	5	3	2	40.
Mechanical means employed: Bone plates	36	28	8	22.2
Turnip plate	2	1	1	50.
Robinson's segmented rubber plate	2	1	1	50.
Mayo Robson bone bobbin	2	1	1	50.
Murphy button	7	6	1	14.3
Suture with mechanical aid—means employed,				
Abbe catgut rings	5	0	5	100.
Brokaw segmented rubber rings	2	1	1	50.
Elastic ligature	1	0	1	100.

CAUSES OF DEATH BY GASTRO-ENTEROSTOMY.

By all methods:

Shock, 4.	Hæmorrhage, 2.
Exhaustion, 13.	Complications, 2.
Peritonitis, 4.	Unknown, 4.
Ileus, 1.	

It has been found that, with all other methods except the button, cicatricial contraction occurred to a greater degree in gastro-enterostomy by lateral approximation than in any other operation. To avoid the stenosis that might possibly occur I have devised an oblong button for this class of cases. An opening from two to three inches long can be made with this button, and still its greatest diameter is only five-eighths of an inch, or the same as the diameter of the smallest button, and would be passed as readily.

Contraction.—In the post-mortem records we find reports of complete closure of the opening from cicatricial contraction in five cases; these were all performed by Senn's method with bone plates—that is, of the twenty-

eight cases that survived the immediate effects of the operation, five of the number on whom autopsies were subsequently made showed a complete closure of the opening. To this must be added Mr. Larkin's case in which stenosis occurred, requiring a secondary jejunostomy, making six closures in all, and representing 20.7 per cent of the twenty-eight patients recovering from the immediate effects of operation. As many of them have not had autopsies, it is fair to presume that the percentage of stenosis by this method is very much greater than this. Of these twenty-eight cases the total number in which autopsies were reported is eight. Three of these

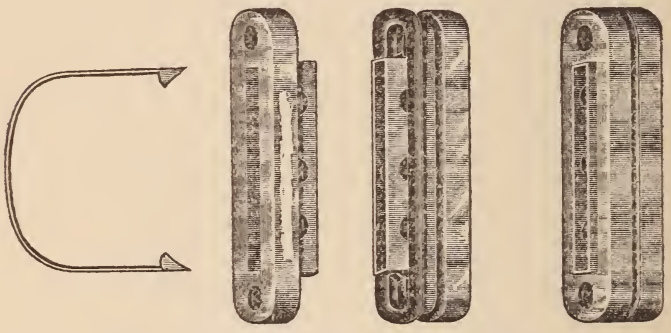


FIG. 4.—Oblong button for lateral anastomosis.

died before the end of four weeks, and the opening was patent; in the remaining five the opening was closed. The time between operation and death was as follows: three weeks, six weeks, four months, six months, and seven and a half months, making eighty-three per cent of stenosis in autopsies where the patients survived three weeks. This would appear to be a very forcible objection to the use of this method in gastro-enterostomy, and should exclude its application.

The lateral approximation by means of the button is the only method in which the aperture is produced by a removal of tissue, all other methods depending upon simple incision.— The cases of stenosis of pylorus from other causes than

malignant disease are of rare occurrence, and are the ones in which it is important to have an approximation made where the element of contraction will not be of moment for an indefinite period of time. Number of this class:

	Cases.	Recoveries.	Deaths.	Percentage of mortality.
Total	37	32	5	13.5
Suture	22	18	4	18.2
Mechanical means	11	10	1	9.09
Of these there were bone plates	10	10	0	0
Swedish turnip	1	0	1	100.0
Suture aided by mechanical means	4	4	0	0

The last four were all by Abbe's catgut rings.

Cause of death: exhaustion, 1; ligature of hepatic artery, 1; perforation at seat of suture, 1; diarrhœa, 1; obstruction, 1.

The operation that should be performed in this class of cases is complete separation of the end of the duodenum and stomach at the site of constriction. The end of the stomach or duodenum should be closed with a Czerny-Lembert suture, and the distal end of duodenum sutured or buttoned to the side of the stomach, thereby avoiding all of the disagreeable and dangerous conditions that accompany gastro-enterostomy by lateral approximation.

PYLORECTOMY WITH GASTRO-ENTEROSTOMY.

	Cases.	Recoveries.	Deaths.	Per cent.
Total	27	16	11	40.7
Suture	24	13	11	45.8
Mechanical means	2	2	0	0
Suture with mechanical aid	1	[1]1	0	0

End of duodenum to side of stomach, 4; recovered, 2; deaths, 2.

[1] Abbe rings.

Cause of death: shock, 3; exhaustion, 2; peritonitis, 5; unknown, 1.

To Rydygier belongs the honor of being the first to perform this operation. Billroth advanced the operation to great prominence. The *modus operandi* of the various operators is practically the same up to the stage of suturing the end of stomach. In Billroth's operation the stomach was sutured from the lesser curvature down until only sufficient opening remained to admit of the insertion of the end of the duodenum. The end of the duodenum was then united by a Czerny-Lembert suture to the opening in the end of the stomach. The time occupied for the operation averaged two and a half hours (Von Baracz), the greater portion being consumed in uniting the duodenum.

Rawdon's operation was practically the same, except that bone plates were used to approximate the end of the duodenum to end of the stomach. The time consumed in the operation by the Billroth method was so great that all of the patients were collapsed before it was completed. Some of them rallied.

There was another danger: it was found that at the junction of the line of suture at the end of stomach with the circle of suture approximating the duodenum, gangrene occurred from the strangulation produced by the suture, and a failure of union resulted, increasing the mortality, already great from shock.

To avoid these calamities it was suggested to close the end of stomach and end of duodenum entirely by suture and make a lateral approximation of the stomach to the duodenum or jejunum. This has been performed twenty-four times with thirteen recoveries (eleven deaths), or 45.8 per cent. It is safer and more rapid than any means used for the approximation of the end of the duodenum to the end of the stomach.

A still better method is the approximation of the end of the duodenum to the posterior wall of stomach through an incision made for that purpose. Number of this class

operated upon by mechanical means, two (anastomosis button); both recovered.

The procedure by this method should be as follows: Place a double clamp on stomach and incise the stomach between, also a double clamp on duodenum with incision between. Transfix the mesentery attached to the pyloric portion with a double-threaded needle; ligate as you would a broad pedicle; excise; continuous Czerny suture of end of stomach; remove clamp and insert continuous Lembert suture, entirely closing the end of stomach. Insert female half of button in posterior wall of stomach as usual, with male half in end of duodenum; press the button together. The operation can be performed in from thirty to forty minutes. It leaves the end of stomach securely closed and the end of duodenum firmly approximated to peritoneal surface of stomach—conditions which are favorable for immediate union. This operation has the following advantages:

1. The danger of shock is very materially lessened by the great reduction of time—*i.e.*, from two and a half hours down to from thirty to forty minutes.

2. The end of stomach is securely closed.

3. There is a uniform pressure in approximation of duodenum to stomach.

4. The danger of peritonitis is lessened, because the peritoneal surface has not been exposed to the atmosphere nor manipulated sufficiently to abrade the endothelial layer on its surface, consequently the likelihood of infection is lessened.

CASE I.—Was one of Dr. Alexander Hugh Ferguson's, Winnipeg, Manitoba, who writes as follows: "I used your 'anastomosis button' four days ago in a gastro-duodenostomy (end-to-side), after pylorectomy for cancer. The time, from taking the knife to complete closure of abdomen, one hour and twenty minutes. Since the operation there has been no vomiting, no tympanites, restlessness, nor pyrexia. The pulse at its highest was 108. The largest-size button suited admirably and I am sure shortened the time one-half." August 14th: "My

case of pylorectomy has made an uninterrupted recovery. The button was voided sixteen days and ten hours after operation. He is now eating meat, potatoes, etc., which he has not done for five years."

September 1st: "Patient discharged from hospital on twentieth day, apparently in perfect health. He had gained five pounds since the operation."

December 28th: "Patient has gained fifty pounds since operation."

Cholecystenterostomy.—Probably the most difficult of all approximations to make by the suture is that of uniting the gall bladder to the intestinal tract. The results from experimental research by this method were very unsatisfactory, and the verdict of Gaston, Harley, Colzi, De Paige, and others who pursued these experiments industriously, was that it was a hazardous operation and one very difficult of execution. This opinion was verified by the difficulties and dangers experienced by the pioneers of this operation. It took Von Winiwarter sixteen months, with six operations, to perform a successful cholecystocolostomy.

In commenting on cholecystenterostomy by means of suture, Kappeler, Fritsche, Robson, Courvoisier, Socin, Bardenheuer, and others say it is an operation very difficult of performance. The number of operations performed by means of suture, up to the present time, is twenty-three, with eight deaths, a mortality of 34.8 per cent; number of operations performed by anastomosis button for cholelithiasis, twenty-three, with twenty-three recoveries.

Number of operations in cases of cancer involving pancreas, duodenum, gall ducts, and liver, two, with two deaths. The death in one case, reported by Dr. Weir, was due to the disease and not to the method, and occurred two hours after operation; in the other, a case of my own, to a twisting of the bowel upon itself before the approximation was made, as shown on autopsy.

A comparison of these results shows that the operation by mechanical means produces most excellent results—in

fact, all that could be desired. It needs no other comment. The explanations for this favorable showing are:

1. That the operation can be performed in an extremely short time (in less than twenty minutes) compared to that by suture, thereby lessening the danger from shock and also of peritoneal infection from prolonged manipulation and exposure.

2. There is a uniform coaptation of gall bladder to duodenum which is very favorable to, or, better still, insures a union, as there has not been in the human subject, nor in the animals experimented on by this method, a single failure of union. The pressure also prevents hæmorrhage, which is one of the dangers of suture, as two patients died from that cause.

3. There is an immediate restoration of bile to the alimentary canal, and an assurance that none of it will escape into the peritoneal cavity at the line of approximation.

I believe that the operation of cholecystenterostomy by mechanical means will, in suitable cases, supersede all other operations on the gall bladder. (See *Medical Record* for February, 1894, for detailed report of first series of these cases.)

Entero-enterostomy.—In treating this subject we will not take up the various methods of approximation that are now only of historic interest, but will consider the methods that have been employed and reported in operations in the last six years, as will be seen in the tabulated statement. These methods we will arrange as in the previous classification: (1) simple suture, (2) mechanical means, and (3) suture with mechanical aid. The pathological conditions which create the necessity for a resection of a portion of the bowel are of three classes. The order of frequency in which they occur is (1) strangulated hernia; (2) ileus (intestinal obstruction), (*a*) strangulation, (*b*) obturation; (3) intestinal perforation, (*a*) traumatic, (*b*) pathological; (4) neoplasms.

There will always be a considerable percentage of mortality from the intrinsic dangers of the pathological con-

ditions, aside from the defects of the means employed for their relief. Therefore the problems to be solved are:

1. How can we reduce the intrinsic dangers of the disease, and

2. How can we minimize the difficulty and danger of the operative procedure *per se?*

In order to decide the first problem—that is, the intrinsic danger—the subject of diagnosis must be exhaustively considered, as the dangers of the disease depend in a large measure upon the failure of early diagnosis. This is particularly true of ileus, and I feel constrained to further emphasize, in connection with this subject, the imperative importance of early and accurate diagnosis, as the result in operations of this class depends more than in any other upon an early recognition of the pathological condition. The difficulty of making a positive diagnosis of the pathological lesion producing the obstruction has been materially diminished by our modern advancements, but is not generally understood nor practised. This necessarily increases the danger to the patient, as the operation is postponed until extreme symptoms manifest themselves. With exploratory laparotomy for ileus but little difficulty is encountered, although it is occasionally impossible to locate the seat of obstruction. In one hundred and ninety laparotomies for ileus collected by Schramm, the operators failed in eight cases to locate the obstruction.

Diagnosis.—There is only one way to obtain good results in ileus—that is, by early diagnosis and operation. In this we must consider every case of ileus as we would an acute strangulated hernia. We know that an early operation for strangulated hernia admits of a much more favorable prognosis than one which is delayed until the intestine is gangrenous. How much more important is it that the early operation should be performed in internal strangulation, where a gangrene would take place within the peritoneal cavity! What physician would allow a strangulated hernia to go unrelieved one night?

On the other hand, what is the practice of physicians in cases of internal strangulation?

They allow not only one night, but two, three, four days and nights to elapse—yes, even to the stage of collapse—before the strangulated bowel is relieved. Only a few days ago I was called to operate in a case of recognized intestinal obstruction of six days' standing; there was not the slightest sign of pulse; the patient's extremities were cold to the trunk; every other respiration was that prolonged, heavy sigh that we find in these cases preceding dissolution. It is needless to say no operation was performed.

This is what is called conservatism; criminal procrastination is a more proper term. Can we expect, under such circumstances, good results from any method of operation? Do statistics offer sufficient inducement under the treatment by opiates, stomach and bowel irrigations, etc., to justify us in waiting, depending upon a favorable accident to rescue the patient? No. *Expectant treatment in abdominal lesions is only justifiable when the diagnosis of intestinal obstruction is positively excluded.* The only rational outcome to expect from so-called conservative treatment of ileus is death.

The differential diagnosis of intestinal obstruction it is not very difficult to make, if the symptoms have not been masked by the use of opiates.

To assist in the diagnosis I submit the following excellent synopsis from Von Zoege-Manteuffel:

I. STRANGULATION ILEUS.

Pathological Changes.	Clinical Symptoms.
1. Localized meteorismus, dissolution of the strangulated coil.	1. (*a*) Asymmetry of the abdomen; (*b*) localized increased resistance.
2. Ischæmia, paralysis of the strangulated loop of intestine.	2. Complete inactivity of the abdominal wall over the strangulated coil, absence of peristalsis.

In this category are included:

1. Volvulus, agglutination, twisting of the intestine on the axis of its mesentery.

2. Strangulation by bands and diverticula.
3. Incarceration in preformed openings.
4. Invagination.

2. OBTURATION ILEUS.

Pathological Conditions.	Clinical Symptoms.
1. Meteorismus produced by accumulation above the obstruction.	1. Perceptible asymmetry, palpable resistance in obstruction of the large intestine. Obstruction of the small intestine, diffuse tympanites.
2. (*a*) No pronounced disturbance of circulation. (*b*) Hypertrophy of the intestinal muscles above the obstruction in the chronic form in the large intestine.	2. (*a*) Peristalsis to be observed or felt. (*b*) Greatly exaggerated peristalsis.

To this group belong:

1. Strictures.
2. Twisting of the intestinal axis.
3. Obstruction by neoplasms or foreign bodies.
4. Compression by tumors from without.

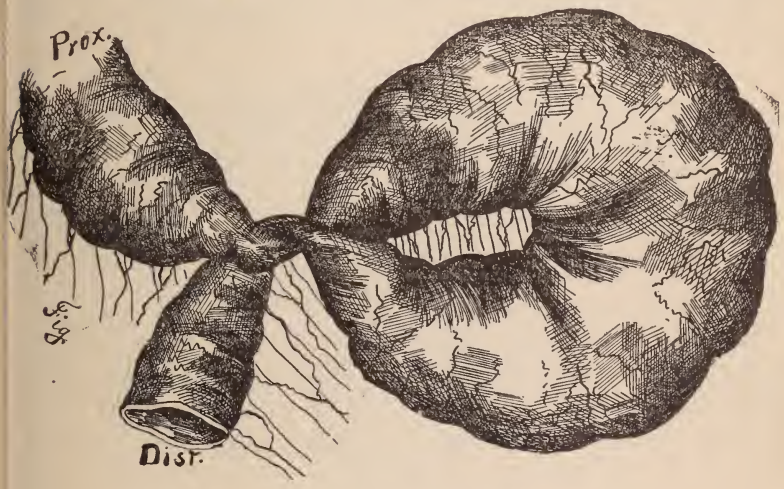
It is a fact that in certain forms of intestinal occlusion (strangulation ileus in contrast with obturation ileus) a definite portion of the intestine suffers from an abnormal fixation and distention, which on inspection can be recognized by the asymmetry of the abdomen, and determined and outlined by careful palpation.

The one pathological condition from which it is most difficult to differentiate ileus is perforative peritonitis, but there are two symptoms upon which great reliance may be placed:

1. In the early stage of perforative peritonitis there is an elevation of temperature; in intestinal obstruction, no elevation of temperature.

2. In perforative peritonitis there is a paralysis of peristalsis over inflamed area; in intestinal obstruction there is a very great increase in the peristalsis, which continues until necrosis of the bowel takes place or a peritonitis is developed. Frequent auscultation of the abdomen as an aid to diagnosis of intestinal obstruction is of as much

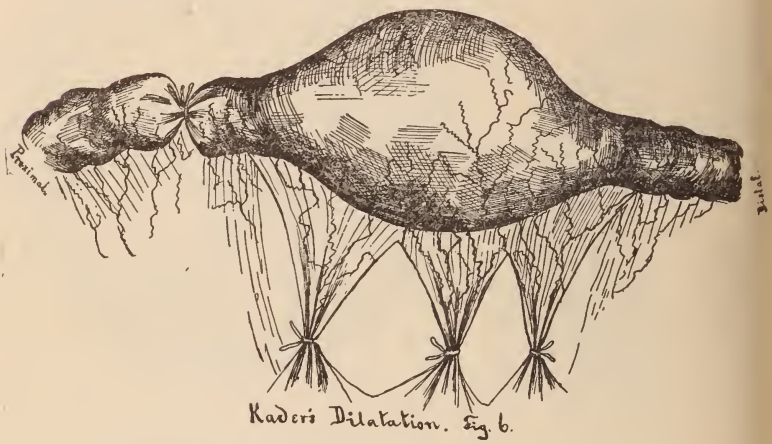
value as auscultation of the chest in arriving at a diagnosis in pulmonary diseases. With the stethoscope or ear, upon the abdomen in the early stage of intestinal obstruction, there is heard a constant rumbling and rolling of the contents of the intestine and manifestations of stormy peristaltic action. These manifestations are most marked at the seat of obstruction, and, indeed, the location of the obstruction can frequently be ascertained by



Dilated loop in Volvulus. Fig. 5.

these signs alone. If this peristalsis should momentarily subside it can be brought on with increased violence by manipulation of the abdomen. A dose of opium will suppress the peristaltic action of the intestine for hours, while a chloroform narcosis does not interfere with it. It should therefore be impressed on every physician that no opiates should be given in acute lesions in the peritoneal cavity until the question of diagnosis is decided. In ileus, where active peristalsis has subsided and cannot be reproduced by pressure, it is an indication that peritonitis has ensued and the prognosis of the operation is pro-

portionately grave. A symptom of importance in obstruction of the bowel is a very circumscribed dulness near or at the seat of obstruction, due to accumulation in the proximal portion of the bowel (Schede, Schlange); and a meteoric distention of the strangulated loop itself, due to obstructed circulation and decomposition of its contents, producing a rapid, gaseous distention and complete paralysis of the wall (Von Wahl) (see Fig. 5). Kader's experiments show that the distention of the strangulated loop is not due to gas and intestinal contents



forced into the loop from the proximal side, as was formerly supposed, but is principally due to disturbances of the circulation in the intestinal wall, and consists of a uniform expansion of the entire intestine, which is produced by (1) thickening of the wall, (2) accumulations of fluid, (3) development of gas from fermentation within the loop. This condition he produced experimentally by ligation of the mesenteric vessels after a ligature of the intestine on the proximal side of the portion in which the circulation was obstructed, the distal end being free, still the intestine dilated as shown in Fig. 6. This has great bearing in all operations of intestinal approximation, as it accounts for the paralysis and distention occurring where a greater

portion of the mesenteric side of the bowel is removed than on the convex side (see Fig. 7).

The outlines of dulness in various intestinal obstructions given by Von Zoege-Manteuffel, cuts of which are interesting and instructive, are here reproduced (see plates 8 and 9). An irregularity on the surface of the abdomen, supported by an increased circumscribed resistance and tension of the underlying coil of intestine, points definitely to an obstruction at the seat of that distended coil—a local meteorism (L. Rehn).

Result of faulty Excision.— Fig. 7.

It will be admitted that a definite diagnosis of the *pathological condition* producing the obstruction may not be possible.

Does that prevent us from operating? No.

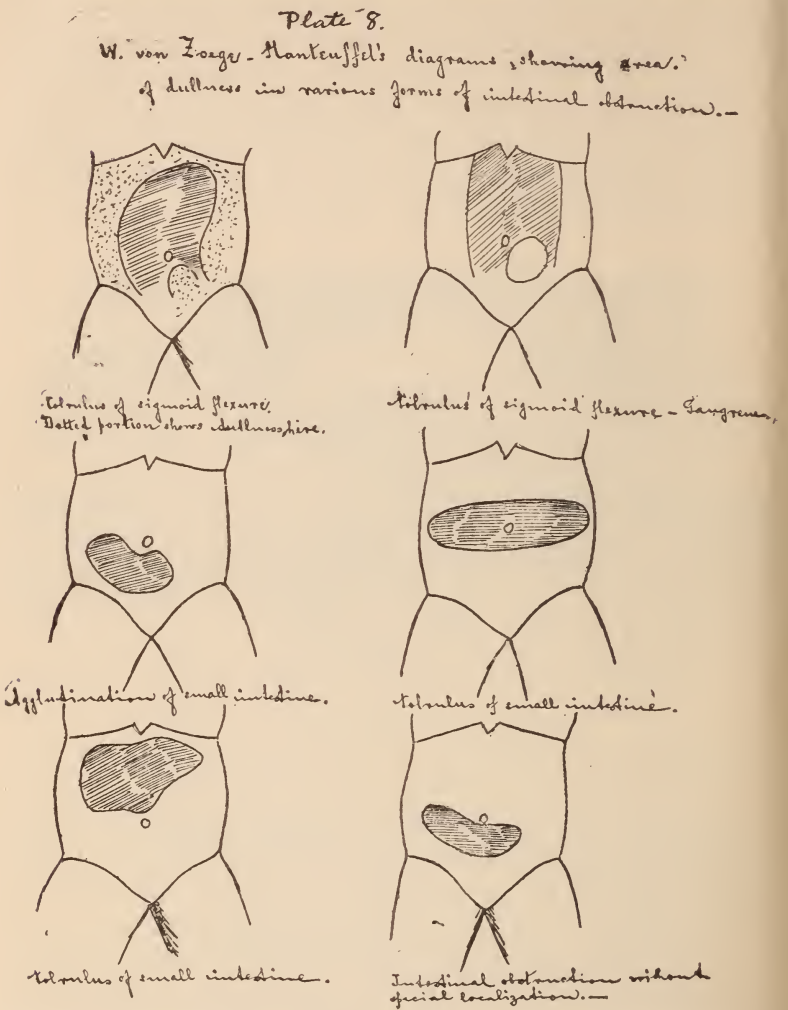
Will waiting help the patient, or will it even assist in making the diagnosis? No.

Every hour lessens the patient's resistance and increases local destruction or necrosis.

Still every day we meet with cases in which this policy of procrastination is pursued until the patient is mori-

bund; finally, and unfortunately too often, we are forced to operate under just these unfavorable circumstances.

When we operate under these conditions are we war-



ranted in abandoning the search for the cause of obstruction and its removal, for the establishment of an artificial anus? I think not.

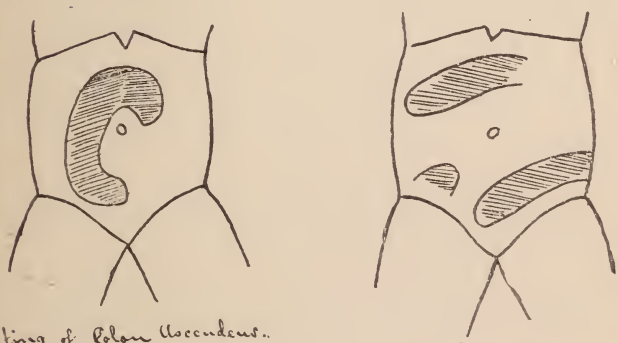
Saltzman states that laparotomies for ileus, with remo-

val of cause of obstruction, up to 1883 had a mortality of 71.3 per cent; simple enterotomy, without removal of

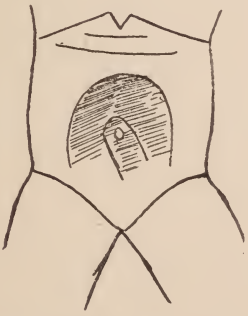
Plate 9.

W. van Zoege — Manteuffel's Diagrams. —

Obturation Ileus. —



Twisting of Colon Ascendens.
Cicatricial occlusion. — Carcinoma flexurae sigmoideae. —



Obturationileus. —

cause, 67.3 per cent. So hopeless were the results of laparotomies with the removal of cause that such surgeons as Mikulicz, Schede, and Shoenborn, who were

formerly strong advocates of this operation, abandoned it for simple enterotomy to relieve the obstruction. The indications for the latter operation they attempted to extend, even with a slight percentage in its favor and the comparative certainty that a second operation would be necessary. The establishment of a fæcal fistula in this class of cases should be permitted only as a last resort in a profoundly collapsed patient, as we have no means at hand of removing the necrotic intestine and re-establishing its continuity in a much shorter time than a fæcal fistula can be formed. The more acute the attack and persistent the pain, the more pronounced the collapse, the more probable the existence of complete strangulation and local necrosis. There is, however, no positive symptom of necrosis.

If the foregoing principles are adhered to the intrinsic dangers of intestinal obstruction can be reduced to a minimum.

As a matter of necessity the surgeon must see the case early, examine it carefully, and must be allowed to name the time of operation.

If we were asked what we considered the greatest danger of intestinal obstruction, would the answer not be, not the ileus, not the gangrene, not the peritonitis, but the lamentable failure to make an early and correct diagnosis, or, still worse, the lack of courage, having made it, to act upon its indications? In other words, the fault is not in defects in Nature, whereby these accidents are permitted to occur, but in the doctor for allowing the lesions to progress unrecognized or unrelieved beyond the possibility of rescue by surgical interference.

Leichtenstern, Peyrot, and Treves have endeavored, through careful analysis and classification of the history, symptoms, and physical signs, to make a definite diagnosis of the nature of the mechanical obstruction and of the portion of intestine obstructed. Leichtenstern expresses his views as follows: "The object of our diagnosis before the operation is performed is to designate the ana-

tomical and pathological conditions by the history, symptoms, and physical signs, and thereby to ascertain not only that an obstruction exists, but also the position, cause, and nature of the obstruction." In a few cases this has been accomplished, but in the great majority it was impossible to arrive at such conclusions with any degree of certainty, and I feel that Schede has expressed the condition exactly as it exists, and includes all that is of vital importance to operators, in the following words: "The greatest difficulty rests, and will for a long time rest, in the diagnosis. I will not even once mention, in considering the diagnosis, the nature of the obstruction nor the portion of the intestine obstructed. I will be satisfied if in a given case, at a given time, the presence of an obstruction to the passage of fæces can be determined, which obstruction it is impossible to relieve by any other means than the surgeon's knife."

Dr. P. Poppert, of Giesen, comments as follows on the cause of death after operation for ileus: "The majority of cases of ileus are already severely depressed—yes, collapsed—when they come for operation; for this kind of depressed patients every long operative procedure, which is accomplished by extensive opening of abdomen and exposure of its contents, will prove fatal, consequently a large percentage of laparotomies of this class die shortly after the operation from heart failure."

LATERAL APPROXIMATION.

	Cases.	Recoveries.	Deaths.	Not given.	Percentage of mortality.
Total	66	43	21	1	31.7
Suture	33	20	12	..	36.4
Suture with mechanical aid	23	18	5	..	21.7
Of these, bone plates	14	11	3	..	21.4
Robson's bone bobbin	1	1	..	..	..
Potato plates	1	..	1	..	100.0
Robinson's raw-hide segmented plates	1	..	1	..	100.0
Abbe rings	4	1	3	..	75.0
Davis mats	1	1	1	..	100.0
Mechanical means: Murphy button	10	10	..	..	..

The operation of lateral approximation (anastomosis proper) is distinctly a modern operation, as it was first successfully performed by Von Hacker, of Vienna, in 1886. It rapidly gained adherents on account of the great mortality following the operation of circular enterorrhaphy, and on account of the many mechanical devices that were brought forward about that time which were intended to simplify the operation—such as the plates used so extensively by Dr. Senn (originally suggested by Connell), Abbe's rings, Robinson's raw-hide and segmented rubber plates, Davis' catgut mats, etc.—and finally on account of the larger aperture that could be secured by the lateral suture.

The objections to this operation after resection are:

1. The amount of time and labor required, as both ends have to be closed with two rows of sutures, each row at least an inch long, necessitating four inches of suturing for the end, no matter what method may subsequently be used for the lateral apposition; then the approximation plates, or button, are inserted. If the suture be used it is advised (Abbe) that an opening four inches long be made, and around this be placed one row of Czerny and two rows of Lembert sutures; these rows will average about ten inches each, which, with the four inches of suturing required to close the ends, make thirty-four inches of suturing. For each inch four to six stitches are required, making a total of one hundred and thirty-six to two hundred and four stitches. An expert operator is able to do this operation in a comparatively short time, as Abbe has done, but how long would it take the average operator?

2. The danger of necrosis of the inverted portion at some point over this long space, which objection is of very small moment.

3. The danger that one of the many stitches might penetrate the thin mucosa—as they must all include the tunica propria upon which it rests—and be exposed in the calibre of the bowel, producing stitch abscess (as in W. Rindfleisch's cases). The silk remains for an indefinite time.

4. The broad cicatricial mass and the one-half inch of surface approximation produced by these three rows of sutures might be expected to contract, as there is no juxtaposition of the incised edges of the various coats of the intestine. This is shown in the case of Salzwedel,

Rockwitz' position, Fig. 10.

lateral approximation of intestine with suture, and still it is the *only* case we found of post-operative stenosis of the intestine, showing that it is of very rare occurrence.

End-to-Side Approximation.—There are two places in the intestinal tract where this operation should always be given the preference: (1) in resection for cicatricial occlusion of the pylorus the end of the duodenum should be

joined to the side of the stomach; (2) resection of the cæcum in adults on account of the inequality of size of colon and ileum.

In the first case the food is allowed to enter the intestine in about its normal position, and duodenal digestion progresses about the same as if no operation had been performed; of still greater importance is the fact that by this method the bile and pancreatic secretions are not permitted to enter the stomach and cause such grave disturbances as are reported to have occurred (Dr. Heinrich Braun). He considered the condition so grave that he suggested an operation involving triple anastomosis to prevent the bile from entering the stomach (see Fig. 10). Lauenstein recommends this operation. End-to-side approximation has no other legitimate place in surgery of the small intestine after resection.

End-to-End Approximation.—We note from the reports of cases in the last six years one hundred and thirty-four intestinal approximations by all methods, of which forty-nine were end-to-end, sixty-one side-to-side, and twenty-four in which the position of approximation was not stated.[1] Of these, forty-four cases were intestinal approximation for acute obstruction, with twenty-nine recoveries and fifteen deaths, mortality 34.1 per cent; for chronic obstruction, forty-three cases with thirty-one recoveries and twelve deaths, mortality 27.9 per cent; making a total for obstruction of eighty-seven cases with sixty recoveries and twenty-seven deaths, a mortality of 31 per cent. The methods employed in acute obstruction were: Suture, twenty-eight; recoveries eighteen, deaths ten; mortality, 35.6 per cent. Mechanical means, thirteen; recoveries ten, deaths three; mortality, 23.1 per cent. Suture with mechanical aid, three cases with one recovery and two deaths; mortality, 66.7 per cent. Methods employed in chronic obstruction were: Suture, twenty-eight cases; nineteen recoveries, nine deaths; mortality, 32.1 per cent.

[1] Cases were added since the table of percentage was made.

Mechanical means, thirteen cases; eleven recoveries, two deaths; mortality, 15.4 per cent. Suture with mechanical aid, two cases; one recovery, one death; mortality, 50 per cent. Total number of intestinal approximations for closure of artificial anus, thirty-five; twenty-eight recoveries, six deaths, one unknown; mortality, 17.1 per cent. Of these twenty-four were suture, with eighteen recoveries, five deaths, one unknown, mortality 40.8 per cent; seven were by mechanical means, seven recoveries; four were by suture with mechanical means, three recoveries, one death, mortality 25 per cent.

End-to-end approximation of the bowel is the most desirable position, as it most closely approaches the natural condition. Surgeons have always regarded the end-to-end as the ideal approximation. This was only deviated from with the hope of reducing the great mortality that resulted with the methods heretofore employed. Has this deviation accomplished the desired result? Our statistics answer no. In forty-nine cases of end-to-end approximation by all methods the above statistics show a mortality of 16.3 per cent, while in sixty-six lateral approximations by all methods the mortality was 31.8 per cent. Results of the lateral approximation therefore show a mortality about 100 per cent higher than that of end-to-end approximation.

Paul remarks that "end-to-end is preferable to lateral union, if it can be obtained with equal safety." The objections raised to the circular suture are, *first*, the time consumed; *second*, the possibility of perforation in parts not covered with peritoneum (mesenteric attachment); *third*, the large number of sutures and the liability that some of them would go through the entire thickness of the bowel and produce peritoneal infection; *fourth*, with the invagination method there is the additional danger of post-operative progression of the invagination, finally producing obstruction; *fifth*, the danger of obstruction from contraction of the necessarily broad cicatrix.

While these objections have force against the end-to-

end approximation by suture, not one of them is valid against the use of the button.

Abbe favors the lateral approximation by suture in the following words: "Lateral anastomosis, properly done, is eminently the safest and best method of restoring the canal in most cases." The statistics of lateral anastomosis do not sustain this statement. Our statistics show a mortality in lateral approximation of 31.8 per cent; end-to end approximation, 16.3 per cent.; of this the mortality with suture was 20.6 per cent.

The first objection raised to the button was that it is a foreign body in the intestinal tract and might cause obstruction. This accident has not occurred in one of the sixty cases operated upon, nor has it taken place in over three hundred experiments; nor will it occur except where there is a pathological contraction of bowel. The second objection, that the necrosis of the tissue within the clasp of the button would be a source of danger, is purely mythical, as the bowel is securely sealed *par excellence* by this method. Infection has not occurred in a single case. The third objection, that the small surface of apposition would not prove adequate in the protection of the peritoneal cavity, has as little basis as there is for believing that the wall of the intestine itself is not strong enough to protect the peritoneum, as the pathologists show in slides, herewith submitted, that the intestine at the seat of approximation can scarcely be differentiated from the normal intestine. I have received no information, nor do I know of a case in which there was a failure of union by the button, nor do I believe that such failure will occur except where there is a septic peritonitis *due to infection from without.* The contraction of three-eighths of an inch in the opening reported by Keen in his case is in unison with the contraction of the bowel from its previously distended condition and should be expected under these circumstances. But there is no proof that the contraction would have continued to complete closure, as occurred with the bone plates. Two years have elapsed since the

first operation with the button, in many others over a year, and in a still greater number a shorter period of time. I have received recent reports on most of these cases, and stenosis has not occurred in a single one.

I will give a synopsis of the cases of intestinal approximation as furnished me by the operators:

CASE I.—From Dr. H. O. Walker, of Detroit, Mich. Diagnosis: Fæcal fistula following laparotomy. Several unsuccessful attempts by other surgeons had been made to close fistula; laparotomy December 8th, 1892; excision of about four inches of portion of bowel involved in fistula; end-to-end approximation by Murphy button; time consumed in performing operation, seven minutes; patient had uneventful convalescence, and in a letter received from Dr. Walker this month he states that she has been in excellent health since operation, about one year ago. This was the first case of end-to-end approximation performed on human subject with Murphy button.

CASE II.—From Dr. Hoelscher. Mrs. L., æt. thirty-five. Diagnosis: annular stricture of rectum, situated three inches above the sphincter, which would scarcely admit the tip of a finger. Operation January 14th, 1893, by Dr. Murphy. The stricture was split down to the bone, half of the Murphy button passed up above the stricture. Incision united with sutures. Other half of button placed below the stricture and button pressed together. This produced a necrosis of the cicatricial tissue constituting the stricture, and an end-to-end union of the rectum. The special button used was one and a half inches in diameter, and was voided on the twelfth day. Patient was seen three months after operation, and there was but little contraction.

CASE III.—From Dr. W. W. Keen, of Philadelphia. Mrs. P. Diagnosis: carcinoma of hepatic flexure of colon. Operation January 30th, 1893, for repair of artificial anus and re-establishment of continuity of the bowel. Enterectomy of portion of bowel involved in fistula; suture of distal end of ileum with Cushing's right-angle suture; united proximal end of ileum to side of colon with Murphy button (middle size); closed abdomen; recovery. In writing on this method Keen says: "The speed and certainty with which an anastomosis can be made, once that the bowel is prepared for it, are certainly advantages

which the button possesses over every other means of anastomosis, whether by simple suturing or by bone plates, catgut or other rings. The question of speed in such abdominal operations is of the utmost importance, and this device is by far the quickest of all means of anastomosis." For detailed report of this case see *Annals of Surgery*, June, 1893. This was the first case of end-to-side approximation by the Murphy button.

CASE IV.—From Dr. G. D. Thomas, of Chicora, Pa. Mrs. H., æt. fifty. Diagnosis: fæcal fistula with stricture of bowel. Operation March 25th, 1893, by Dr. R. S. Sutton, of Pittsburg. Lateral approximation was made with the Murphy button (largest size); bowels moved twenty-four hours after operation; on and after the sixth day bowels moved regularly; the constipation from which the patient had suffered having been overcome, fæcal fistula closed in three weeks without operation.

August 11th, 1893, patient continued in excellent health, being robust and going about her household duties. This was the first case of lateral approximation by Murphy button.

CASE V.—From Dr. E. Wyllys Andrews, Chicago. Mrs. F. F., aged thirty-eight. Diagnosis: strangulated right femoral hernia. April 3d, herniotomy; gangrene of eighteen inches of ileum; excision; end-to-end approximation with Murphy button (middle size); mass dropped into the abdomen; procedure rapid and easy; no drainage; bowels moved fifth day; nineteenth day button voided in normal stool; patient discharged April 23d, 1893. November, 1893, patient in perfect health.

CASE VI.—Dr. Murphy. Mrs. E. T. S., æt. fifty-three, in consultation with Dr. Fortier. Diagnosis: dermoid cyst, intestinal obstruction. Patient removed to Post-Graduate Hospital; laparotomy by Dr. Murphy, June 2d, 1893, three days after onset of symptoms. Intestines found adherent in two places to dermoid, but this did not produce the obstruction; cyst removed; obstruction located, a circular band completely occluding small intestine; two inches of intestine excised; approximation made with Murphy button; mass dropped into abdomen; time for making resection and approximation, five and one-half minutes; time for whole operation, including complete closure of abdomen, nineteen minutes. Patient made an uneventful recovery; temperature never reached 100°; bowels moved every day after operation; button

voided nineteenth day. December 2d, 1893, patient in excellent health.

CASE VII.—Dr. Murphy. Mrs. K., æt. forty-eight, admitted to Post-Graduate Hospital July 10th, 1893. Diagnosis: strangulated umbilical hernia, peritonitis. Laparotomy same day; resection of four inches of gangrenous bowel which had perforated within the sac and contents had escaped into peritoneum. Approximation with Murphy button; time for resection and approximation, seven minutes; extensive peritonitis; gauze drain. Death thirty-six hours after operation. Autopsy one hour later showed perfect agglutination of bowel, so that the exact line of union could not be defined. Specimen exhibited to Post-Graduate class.

CASE VIII.—From Dr. Hugh McCall, Lapeer, Mich. T. H. P., æt. fifty-five. Diagnosis: fistula from gangrenous hernia. Operation July, 1893. Lateral approximation with Murphy button (largest size). On the seventh day had a copious natural bowel movement, the first in seven weeks. The button was perfectly satisfactory in establishing the anastomosis.

CASE IX.—From Dr. Bacon. Mrs. L., æt. thirty-five. Diagnosis: specific stricture of rectum, beginning two inches above sphincter and extending up two inches. Operation July, 1893, modified Kraske; exposure of rectal wall, which was denuded, folded upon itself. Button passed on sixteenth day. There was some hæmorrhage a week or ten days after the button passed. The result is very gratifying. The patient was examined May 20th, 1894; found rectum of ample calibre, and the wall, which was previously dense, is now soft and pliable.

CASE X.—Dr. Murphy, on September 10th, 1893, at the request of Dr. Joseph Price, of Philadelphia, in the latter's private hospital, performed lateral approximation for fæcal fistula on Mrs. K. in the presence of the members of the Pan-American Medical Congress. Cause of fistula, strangulated femoral hernia. Murphy button (largest size) used in making approximation; time from beginning of operation until completion of anastomosis, six minutes.

Under date of October 24th Dr. Price writes: "Your patient made an excellent recovery and has gone home healthy and happy; her bowels act kindly, and the fistula closed without operation; passed button on twelfth day."

CASE XI.—Dr. Murphy. Mrs. B., aged forty-five. Diagnosis: fæcal fistula following laparotomy for tumor. Present at operation, Drs. Mayo and McGahey, and members of the Pan-American Medical Congress. Operation September 26th, 1893. Lateral approximation; time, seven minutes; recovery. Fistula not yet closed.

CASE XII.—From Dr. W. B. Rogers, Memphis, Tenn. Male. Diagnosis: strangulated inguinal hernia. Operation October 12th, 1893; herniotomy; removal of ten inches of gangrenous intestine; end-to-end union with Murphy button (largest size). Button voided on seventh day. Patient up and about in four weeks.

CASE XIII.—From Dr. H. O. Marcy, Boston, Mass. Male. Diagnosis: cancer of upper portion of rectum; profuse and dangerous hæmorrhage; case had had a previous left inguinal colostomy. Operation October 16th, 1893. Removal of coccyx and two-fifths of sacrum. Opened the peritoneum; divided mesorectum, which permitted the bowel to come well down; resected four inches of rectum, and joined it end-to-end with Murphy button (largest size); button removed with slight traction on twelfth day. Patient made an uneventful recovery. Dr. Maurice Richardson congratulated Dr. Marcy on the result obtained in this case, and in commenting on it said: "It would seem to indicate that by this means cases were amenable to treatment that were heretofore inoperable."

CASE XIV.—From Dr. C. E. Ruth, Keokuk, Iowa. A W., aged five and a half years. Diagnosis: tumor of cæcum, obstruction of three months' duration; patient very much emaciated. Operation October 8th, 1893. The entire cæcum with two inches of colon and three-fourths inch of ileum excised, together with the enlarged glands in the neighborhood; approximation of end of ileum to end of colon with Murphy button (smallest size).

Dr. Ruth[1] remarks: "This is the first excision of the cæcum, to my knowledge, in one so young, for intestinal obstruction due to neoplasm, and the first in which the Murphy button has been used to make the end-to-end junction of ileum and colon in the human subject. Pulse at the end of operation, which lasted but a few minutes, 160. Patient made an uneventful recovery, and on November 4th left for his home apparently in the best of health. In spite of the objections made to the Murphy

[1] Tri-State Medical Journal, December, 1893.

button, these stubborn facts remain: (1) It furnishes the strongest junction known; (2) we do not need to wait five days to get strong union; it is immediately obtained with proper approximation of the segments; (3) in urgent cases nourishment can be commenced at once, which would be scarcely thought of in connection with any suture or plate device; (4) no foreign body is left permanently in the walls of the gut or in the peritoneal cavity to cause subsequent trouble; (5) in anastomosis between the gall bladder and duodenum, in the formation of biliary, gastric, or fæcal fistulæ externally, and by Bacon's method of treating non-malignant strictures of the rectum, it leaves nothing to be desired; (6) if circular enterorrhaphy, *undoubtedly the ideal approximation*, is ever justifiable, it is by this means, as it minimizes the objections urged against the operation, viz., weakness at the mesenteric attachment and subsequent contraction; (7) the union can be made in less time than by any other means, thereby greatly lessening the primary mortality. The objection has been urged that the opening through the button is not sufficiently large. In my case the smallest-sized button was used and the immediate symptoms of obstruction were decidedly less than at any time during the three months previous to the operation, and the pain was also less, showing that there is no need to fear on that score. In the small intestines the contents, being fluid, will pass, and of course there will be no difficulty with the gas as far as the button is concerned. In the management of these cases it is presupposed that the surgeon would use ordinary intelligence in feeding after a resection of the intestine, and not diet his patients on food that could not be rendered fluid, or nearly so, in the small intestines. All the alimentary functions seemed perfect after the eighth day. On the fifteenth day he is dressed and walking about, apparently perfectly well. He is rapidly gaining in flesh and strength. Microscopical examination showed the tumor to be a large round-celled sarcoma."

Dr. Ruth's conclusions are based upon very extensive experimental research.

CASE XV.—From Dr. A. I. Bouffleur, Chicago. Female. Diagnosis: intestinal obstruction from strangulated femoral hernia, in which a herniotomy with enterotomy in gangrenous portion had been made two days previous without relieving the obstruction, making six days in all

from beginning of strangulation. Operation November 11th, 1893; laparotomy; excision of four inches of bowel at seat of gangrene and fistula; end-to-end approximation with Murphy button (largest size). Time for resection and approximation, four and a half minutes, the quickest time in which this operation has been performed, notwithstanding the fact that the doctor had not used the button previously. Abdomen closed without drainage. Patient was very much collapsed at time of operation; however, she gradually improved and made an uneventful recovery, having copious passages two days after operation.

CASE XVI.—From Dr. A. H. Cordier, Kansas City, who writes as follows: "On November 16th, 1893, I operated for intestinal strangulation (acute), due to a complete stricture of the ileum from an old inflammatory band. The constriction had existed ten days previous to the operation. I used the button, and am pleased to tell you that his recovery up to this date (November 27th) has been an ideal one. His bowels moved within three hours after the operation. This was my first experience with the button, but I have used rings, mats, decalcified bone plates, etc., and I must say that the button is the quickest, easiest, safest, and surest of anything I have so far tried."

CASE XVII.—From Dr. Carl Beck, of Chicago, September, 1893. Patient æt. thirty-five. Diagnosis: cancer of pylorus, involving liver, pancreas, and mesentery. Maydl's operation, duodeno-jejunostomy, using Murphy button for anastomosis. Patient progressed nicely until the evening of the sixth day, when symptoms of peritonitis began and terminated in collapse the following day.

"Autopsy revealed a peritonitis, another cancer in the sigmoid flexure obstructing the same. The food that had been injected per rectum had ruptured the bowel at this point and caused the fatal peritonitis. The button, which was of excellent use in the operation, was found in place; perfect union over the entire circumference; no omental adhesions around the point of union nor in the neighborhood. I think if it were not for the complication of the second carcinoma the case would have been a perfect success. As to the usefulness of the button it has been a most beautiful illustration."

Conclusions.—1. The more rapidly the operation is performed the less the danger from shock.

2. The less the manipulation and exposure of the in-

testine, the less the danger of infection, post-operative paralysis, and adhesions.

3. The more uniform and continuous the pressure at approximation, the greater the assurance of adhesion and the less the liability of infiltration.

4. A line of approximation is as good as half an inch.

5. Mechanical means in the last six years have produced better results than the suture, in both lateral and end-to-end approximations.

6. The mortality in end-to-end approximation is much less than in lateral apposition, and it should always be given the preference.

7. The more perfect the juxtaposition of the various layers, the less the interposition of fibrous tissue and the more complete the regeneration across the line of union.

8. The more extensive the approximation surface, the larger the fibrous deposit, the greater the contraction.

9. The contraction with end-to-end is less than with lateral approximation.

10. The juxtaposition of the similar histological layers of the wall of the intestine is an assurance against cicatricial contraction.

11. The *modus operandi* of the button is based upon the following principles: (1) It retains apposition automatically (that is, without suture); (2) union of tissue is produced at line of pressure atrophy; (3) the pressure atrophy is produced by elastic pressure; (4) it produces juxtaposition of the edges of same coats; (5) the union is accomplished by the smallest possible cicatrix, and therefore must yield the least contraction of any operation; (6) the rapidity of application gives all the advantage that the saving of time can accomplish.[1]

[1] The editor would refer the reader to the Chicago Clinical Review, 1893–1894, vol. iii., p. 547, for the admirable bibliography belonging to this article, that want of space compelled him to omit.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Recov.	Death.	RESULT. Cause of de'th	REMARKS.
July 4, '87	Medical News, June 11, '92.	Monastyrski.	Carcinoma head of pancreas.	Cholecystojejunostomy.	Suture.	1			Death 4 mo. later.
" 6. '87.	Gaillard's Med, Jour. Aug,, '92.	Kappeler.	Tumor pancreas.	Cholecystoileostomy.	" Wolfler.	1			Death 15 mo. later.
Nov. 19, '87	Correspbl. f.Schweiz. Artze 4, '90.	Socin.	Carcinoma pancreas.	Cholecystenterostomy.	"	1			
Apr 24, '88	Correspbl. f.Schweiz. Artze 6, '90.	Fritzche.	" common duct	Cholecystojejunostomy.	Elas. ligature		1		
" 24, '88	Centbl. f. Chir. 12, '89	Badenheuer.		Cholecystenterostomy.	" "		1		
" 24. '88	" " "	"		"	" "		1		
Mar. 2, '89	Med. Chir. Trans. '90	Robson.	Closure biliary fistula after cholecystotomy.	Cholecystocolostomy.	Suture.	1			
Aug. 13, '89	Rev. de Chir., '92.	Terrier,	Occlusion, common duct	Cholecystoduodenostomy.	Suture&drain tube.	1			Death 1 year later.
Mar. 28. '90	Path. and Chir. d. Gallenwege, '90.	Courvoisier.	Complications following cholecystotomy.	Cholecystenterostomy.	Suture.	1			
Sept. 29, '90	Sammlung Klin. Vortrage. 40, '92.	Korte.	Cholelithiasis obstruction.	Cholecystoduodenostomy.	"		1	After 7 days. Collapse.	Second operation Oct. 3.
Jan. 14, '91	Berlin Klin. Woch., 12, '92.	Lindner.	Closure biliary fistula.	Cholecystenterostomy.	"	1			
" 16, '91	Deutsche Med. Woch. 9-3, '91,	Sprengel,	Cholelithiasis.	Choledochoduodenostomy.	"	1			
May 21, '91	Samml. Klin. Vort. 12, '92.	Korte.	" obstruction,	Cholecystoduodenostomy.	"	1			
Oct. 24, '91	Lancet, March 12, '92	Chavasse,	Closure biliary fistula after cholecystotomy.	Cholecystocolostomy.	Bone plates.				
Nov. 2, '91	Berlin Klin. Woch. 12, '92.	Lindner,	Closure biliary fistula after cholecystotomy.	Cholecystenterostomy.	Sutnre.	1			
Nov. 16, '91	Deutsche Med. Woch. Feb. 25, '92.	Helferich.	Closure biliary fistula after cholecystotomy.	Cholecystojejunostomy.	"	1			
Aug. 13, '92	Bull. et mem. Soc. de Chir., '93.	Reclus.	Cancer of pancreas, probable.	Cholecystenterostomy.	"	1			
" 28, '92	Wien. Klin. Woch., Jan. 19, '93.	Frankel.	Closure biliary fistula.	Cholecystenterostomy.	"		1	After 10 hours	
Sept. 21, 92	Wien. Klin. Woch., May 11, '93.	Gersuny.	Cholelithiasis obstruction.	Cholecystogastrostomy.	"	1			
" 21, 92	Band III, Klin. Chir. IX., p. 2, '92.	Czerny.	Closure biliary fistula after cholecystotomy.	Cholecystenterostomy.	"		1	Exhaustion.	
" —, 92	Band III, Klin. Chir. IX., p. 2, '92.	"	Occlusion,	Cholecystocolostomy.	"		1	2 weeks hæmorrhage.	Temporary recovery.
" —, '92	Band III, Klin. Chir. IX., p. 2, '92.	"	" tumor.	Cholecystoduodenostomy.	"	1			
Jan., '93.	Boston Med. & Surg. Jour., March 23. '93.	Jones.		Cholecystenterostomy,	"	1			
Feb. 1, '93.	Am. Gynec. Jour. June, '93.	Ross.		Cholecystoduodenostomy.	Elas. ligature		1	37 days continuous hæmorrhage.	

Date.	Publication.	Operator.	Diagnosis.	Operation.	Method.	Position.	Recov.	Death.	Remarks.
Jan. 24, '88	Arch. f. Klin. Chir xxxix	Billroth.	Sarcoma, adherent pancreas.	Resection stomach.				1	After 5 hrs., collapse, vena colica ligated.
Feb. 14, '88	N. Y. Med. Jour. June 30, '88.	Lange.	Carcinoma.	Gastro-enterostomy.		S		1	Af. 31 days; perforation wall of stomach at site of ulcer.
M'ch 13, '88	Med. Chir. Trans. '89	Lauenstein.	Cancer, pylorus.	Gastro-enterostomy.	Suture.	S		1	After 11 days; exhaustion.
Apl. 2, '88	Arch. f. Klin. Chir. XXXIX.	Eiselsburg.	Carcinoma. pylorus.	Pylorectomy and gastro enterostomy.	"			1	48 hours; peritonitis.
Apl. 26, '88	" " " "	Billroth.	Sarcoma pyl. and stom.	Resection stomach.	"			1	4 days; "
July 4, '88	" " " "	"	Cicatricial stenosis pyl.	" "	"			1	After 48 hours; peritonitis.
Sept. 7, '88	Wien. Klin Woch. Jan. 31, '89.	Obalinskil	Carcinoma pylorus.	Pylorectomy.	"		1		Death 18 weeks later.
Dec. 4 '88	Arch. f. Klin, Chir. XXXIX.	Billroth.	Sarcoma abdominal wall adherent to stomach.	Resection of stomach	"		1		Vental hernia.
Dec. 16 .88	" " " "	"	Cicatricial sten. pylorus.	" " "	"			1	After 14 days; per. peritoni.
Dec. 21 '88	Med. Chir. Trans. '89	Rydygrei and Jaworski.	Malignant growth of pylorus and stomach.	Gastro-enterostomy.	" Wolfler Barker.	S	1		
Dec. 31 '88	N. Y. Med. Jour. June 22, '89.	Meyer.	Cancer, stomach.	" "	" cont. catgut.	S		1	After 6 days: ileus delirium, wound torn open.
'88	Med. Chir. Trans. '89	Fritzsche.	Malig. sten. of pylorus.	" "	"	S	1		
'88	" " " "	"	Cic. sten. of pylorus.	" "	"	S	1		
Jan. 13 '89	Arch. f. Klin. Chir. XXXIX.	Eiselsburg.	Sarcoma pylorus.	Resection stomach.	"		1		
Apr. 8 '89	" " " "	"	Cic. sten. pylorus.	" "	"		1		
Apr. 12 '89	" " " "	"	Sar. pyl. and panc.	" "	"		1		Death 5 months later.
May 22 '89	" " " "	Billroth.	" " " "	" "	"			1	After 33 hours; collapse.
June 25 '89	" " " "	Eiselberg.	Tum. pyl. ad. liv. pan.	Gastro-enterostomy.	"	S	1		
June 26 '89	Brit. Med. Jour. May 3, '90.	Stokes.	Can. of pyl. Dil. stom.	Pylorectomy and gastro enterostomy.	"			1	After 11½ hours; collapse.
July 7 '89	Arch. f. Klin. Chir. XXXIX.	Salzer.	Tumor involving transv. colon.	Gastro-enterostomy.	"	S	1		
July 13 '89	" " " "	"	Tum pyl. without sten.	" "	"	S		1	After 7 hours: ligation hepatic artery.
Aug. 22 '89	" " " "	"	" "	" "	"	S	1		
Sept. 10 '89	" " " "	Eiselsberg.	" "	" "	"	S		1	After 3 days; perforation of ulcer at point of suture.
'89	Deut. Med. Woch. May 23, '89.	Lauenstein.	Cicatricial stenosis pylorus, ulcer.	" "	" Wolfler.	S	1		
'89	Brit. Med. Jour. Apr. 22, '93.	Moullin.	Growth in duodenum.	" "	"	S	1		
Mch. 22 '90	Wien. Klin. Woch Nov. 3, '92.	Hacker.	Stenosis pylorus.	" "	"	E-S	1		Implantation of duodenum.
May 9, '91	Deut. Med. Woch. Dec. 8, 92.	Mikulicz.	Carcinoma pylorus.	Pylorectomy and cholecystotomy.	"				

May 23, '91	Wien. Med. Presse, Nov. 20, '92.	Schramm.	Tumor pylorus.	Gastro-enterostomy	"	S	1		
July 7, '91	Med. Chir. Trans. '89	Robson.	Carcinoma. pyl. sten.	Pylorectomy.	"			1	After 5 days; exhaustion.
July 15, '91	Wien. Med. Presse. Nov. 20. 92.	Schramm,	Adeno-carcinoma, pylorus.	Gastro-enterostomy.	"	S	1		
Sept. 17 '91	Wien. Stet.. Woch. Nov. 3, '92.	Hacker.	Carcinoma pylorus.	Pylorectomy.	"		1		
Dec. 7, '91	Med. Rec. Apr. 2, '92.	Abbe	Car. pyl. and stomach.	Gastro-enterostomy.	"	S		1	Few hours; exhaustion.
'91	Wien. Med. Presse. Nov. 20, '72.	Schramm.	Tumor pylorus.	" "	" Wolfler Lembert.	S	1		Death 4 mos.: marasmus.
,91	Deut. Med. Woch. Dec. 8, '92.	Mikulicz.	Carcinoma pylorus.	Pylorectomy.	"		1		
'91	" " " "	"	" "	Gastro-enterostomy.	"Czerny-L	S	1		
Jan. 25, '92	Hygeia. Jan. '93	Sandberg.	Cic. sten. pylorus.	(Hacker.)	" Wolfler	S	1		
Jan. 31, '92	Wien. Klin. Woch., Nov. 24, .92.	v. Hacker.	Carcinoma pylorus.	" "	"	E-S		1	6 days. Catarrhal pneumonia. Gangrene.
Feb. 10, ,92	Lo Sperimentale, '92	Colzi.	Cic. sten. pyl. ulcer.	" "	"	S	1		
Mch. 1, '92	Wien. Klin. Woch., Nov. 24, '92.	v. Hacker.	Carcinoma pylorus.	" " implant.	" Wolfler.	S	1		Temporary improvement.
Mch. 21, '92	Lo Sperimentale '92.	Colzi.	Stenosis pylorus.	" "	"	S	1		
Mch. 31, ,92	Wien. Klin. Woch. Nov. 10, '92.	v. Hacker.	Lymphosarcoma, stomach.	" "	"	S	1		
Apr. 29, '92	Lo Sperimentale. '92.	Colzi.	Cic. Sten. pyl.	" "	"	S	1		
May 8, '92	Wien. Med. Woch. No. 1, '93.	Albert.	Stenosis pylorus atrophy muscularis of stom,	" "	" Lembert.	S	1		
May 17, '92	Wien. Klin. Woch. Nov. 24, '92.	v. Hacker.		" ',	"	S			
May 27, '92	Lo Sperimentale, '92.	Colzi.	Carcinoma pylorus.	" "	"	S		1	4 days. Exhaustion.
May 31, '92	Unpublished.	Beck.	" "	" "	" Maydl.	S		1	4 days. Hæmorrhage of pancreas.
June 30 '92	Chic. Med. Recorder, Oct. .92.	"	Cicatricial stenosis, pylorus.	" "		S	1		
July 5, '92	Bull. et. Mem. Soc. de Chir. Nov. '92.	Terrier.	Tumor, pylorus.	" "	"	S	1		Death 2 weeks. Embolism, pulm. artery.
July 12, '92	" " " "	Delaganiere.	Diffuse cancer, pylorus.	" "	"	S		1	5 days. Cachexia.
July 23, '92	" " " "	Reclus.	Stenosis pylorus, dilat.	" "	"	S	1		
Aug. 26, '92	Johns Hopkins Hosp. Bul. May, '93	Finney.	Cicat. ulcer pylorus.	" " Loreta.	,' Halsted.	S		1	15 days. Diarrhœa.
'92	Med. Rev. July 8.	Keen.		" "	"	S	1		
'92	Verh. d. deuts. Gesells. f. Chir., '92.	v. Bramann.		" "	" Wolfler.	S			
'92	" " " "	"		" "	" Courv'r.	S			
'92	" " " "	"		" "	"	S			
'92	Bull. et. Mem. Soc. de Chir', Nov. '92.	Schwartz.	Carcinoma pylorus stenosis.	" "	" Wolfler.	S		1	4 days.
Feb. 24, '93	Gaz. d' Hop. Aug. 1, '93.	Le Bec.	" "	" "	" Terrier.	S	1		
Mch. 1, '93	Ann. surg., July, .93.	Lange.	"	Pylorectomy.	"		1		
Mch. 14, '93	Gaz. de Hop. Aug. 1, 93.	Le Bec.	Carcinoma stomach.	Gastro-euterostomy.	" Terrier.	S	1		
	Brit. Med. Jour., Apr. 22, '93	Barker.	Carcinoma pylorus.	" "				1	Death 53 weeks. Death, some months. Pneu-

GASTRRO-ENTEROSTOMY BY SUTURF.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Posi-tion	Recov.	Death.	REMARKS.
" " " "	"	" "	" "	" "					
" " " "	"	" "	" "	" "				1	Soon after operation.
Oct. 16, '90	Brit. Med. Jour. Mch. 12, '92.	Mc Cormick.	Carcinoma pylorus Scirrhous.	Pylorectomy.	"	E	1		
Oct. 24, '90	Lancet, Jan. 3, .91.	Barrow.	Gastric ulcer.	Gastro-enterostomy.	"	S		1	5 days. Obstruction.
Oct. 28, '90	Daniels Tex. Med. Jour., Jan., '91.	Hadra.	Cancer stomach.	Pylorectomy.	" Czerny-Lembert.		1		
Nov. 21, '90	Munch. Med. Woch., May 24, '93.	Schonborn.	Cancer pylorus, dilatation.	" "	"	E		1	7 days. Exhaustion.
Dec. 19, '90	" " " .92.	"	Cancer pylorus.	" "	"	E	1		5 days; suppurative periton·
Feb. 13, '91	" " " '92.	"	" "	" "	"	E		1	
Mch. 9, '81	Wien. Klin. No. 17 '92.	Maydl.	Cancer stomach.	Resection stomach.	" Maydl.	E-S	1		8 days.
Apr. 11, '91	Hygeia Sept. '91.	Borelius.	" "	Pylorectomy.	"Cz'y-Lem.			1	Death 4 mos.
June 17, '91	Wien. Klin. Woch. No. 26, '92.	Brenner.	Cancer pylorus.	Gastro-enterostomy.	" Brenner.	S	1		
Aug. 18, '90	Hygeia Sept. .91.	Borelius.	" "	" "	" Wolfler.	S		1	5 hours
Dec. 8, '91	Wien. Klin. Woch. No. 19, '92.	Maydl.	" "	" "	" Maydl.	E-S	1		
" '91	Berl. Klin. Woch. Jan 25, '92.	Hadra.	" "	" "	" Hacker.	S	1		" 2 mos.
Jan. 15, '92	Wien. Klin. Woch. No. 26, '92.	Brenner.	" "	" "	" Brenner.	S	1		
Jan. 16, '92	" " "	"	" "	" "	" "	S		1	20 hrs; shock.
Apr. 28, '92	Gaz. d. Hop. June 30, '92.	Michaux.	Cancer stomach.	" "	" Hacker.	S	1		
July 5, '92	Mercrede Med. No. 44, '92.	Terrier.	Cancer pylorus.	" "	"	S	1		

CHOLECYST-ENTEROSTOMIES IN NON-MALIGNANT CASES.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Recov.	Death.	REMARKS.
June 11, 92	N. Y. Med. Rec. Jan. 13, '94.	Murphy.	Cholelithiasis.	Cholecystoduodenostomy.	Murphy button.	1		
Oct. 19, '92	" " " " "	"	"	" "	" "	1		
Nov. 23, 92	" " " " "	"	"	" "	" "	1		
Dec. 18, '92	" " " " "	"	"	" "	" "	1		
Jan. 31, '93	" " " " "	Lee,	"	" "	" "	1		
Feb. 20, '93	" " " " "	Murphy.	"	" "	" "	1		
Apr 6, '93	Jour. Am. Med. Asso., Aug. 26, '93.	Mayo.	Obstruction of common duct.	" "	" "	1		Patient 71 years old.
Apr. 28, 93	Medical Arena, Kansas City.	Foster, W. D.	Gall bladder fistula, obstruct. of choledochus.	" "	" "	1		Fistula closed without operation
May 6, '93	N. Y. Med. Rec. Jan. 13, '94.	Murphy.	Cholelithiasis, occlusion common duct.	" "	" "	1		
" 22, '93	" " " " "	"	Cholecystitis, impaction.	" "	" "	1		
June 1, '93	" " " " "	Fabrique	Hepatic colic, "	" "	" "	1		
" 1, '93	" " " " "	Rogers.	Closure biliary fistula after cholecystotomy.	" "	" "	1		10th day removed button through fistula.
July 18, '93	" " " " "	Hartmann.	Cholelithiasis	" "	" "	1		
Aug. 3, '93	" " " " "	Ferguson.	Dropsy gall bladder.	" "	" "	1		
Sept. 20, '93	" " " " "	Lane.	Cholelithiasis.	" "	" "	1		
Oct. 20, '93	" " " " "	Luken.	Cholelithiasis, obstruction common duct.	" "	" "	1		
" 26, '93	" " " " "	Hartmann.	Cholelithiasis, obstruction cystic duct.	" "	" "	1		
Nov. 7, '93	" " " " "	Murphy.	Cholelithiasis,	" "	" "	1		
Jan. 6, '94	Unpublished.	Dunn.	" obstruction common duct.	" "	" "	1		
Feb. 13, '94	Unpublished.	Murphy.	Cholelithiasis.	" "	" "	1		Button passed 18th day with 26 stones. Two ¾ inch stones removed.
Feb 24, '94	Unpublished	Abbe.	Obstruction of choledochus.	" "	" "	1		
Feb., '94.	Bellevue H. report.	Barrows.	Biliary fistulas.	Cholecystoduoden'tomy	Murphy's but.	1		

CHOLECYSTENTEROSTOMIES IN MALIGNANE CASES.

March, '93	N. Y. Med. Rec., Jan. 13 '94.	Wier.	Cancer of pancreas liver omentum & gall ducts.	Cholecystenterostomy.	Murphy's button.		1	Exhaustion; perfect approximation.
May, '93	" "	Murphy.	Cancer of duodenum, pancreas, gall-ducts and liver.	Cholecystojejunostomy.	" "		1	4 days, ileus from volvulus, loop of jejunum twisted upon itself.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Position.	Recov.	Death.	REMARKS.
May 23, '92.	Verh d. deut. gesell. f. chir., '92.	Braun.	Gangrene; small intest. following herniotomy.	Entero-enterostomy.	Suture.	E	1		
June 16, '92	Ann. Surg.. Sept. '93,	Barton.	Epithelioma, ileo-caecal valve; obstruction.	Enterectomy.	"	S		1	After 4 days: obstruction.
July 18. '92.	N. Y. Med. Jour.. July 22, '93.	Vance.	Strangulated femoral hernia.	Entero-enterostomy.	"	E		1	62 hrs.; Septicaemia.
Nov. 26,'92,	Lancet, Mch. 11, '93.	Treves.	Epithelioma sigm. flex. obstruction.	Colo-colostomy.	"	E	1		
'92.	Proc. Phila. Co.Med. Soc., Apr., '92.	Ashton.		Ileo-ileostomy.		S	1		
Jan. 17, '93.	Med. Rec., Mch 4, '93.	Lange.	Carcinoma iliac flexure.	Colo-colostomy.	"	E		1	10 days:hypostatic pneum.
Jan. 30, '93.	N. Y. Med. Jour., July 22, '93,	Vance.	Strangulated femoral hernia.	Entero-enterostomy.	"	E	1		
Apl. 4, '93.	Jour. Am. Med. Ass. July 29, '93.	Tappey.	Closure artificial anus. hernia.	" "	"	E	1		
	Arch. f. Klin. Chir. 45, '93.	Schlange.	Closure artificial anus her. obs.	" "	"	S	1		
Feb. 15, '88.	" " " 42, '91.	v, Baracz.	Closure artificial anns. hernia.	Resection ileum.	" Czerny Lembert.	E	1		
'88.	Deut. Chir. (Sachs)	Chwab.		Resection jejunum.			1		
"	" "	Barton.	Epithelioma ileo-cæcal valve.	"			1		
"	" "	Hofmokel.	Carcinoma ascend. colon.	"			1		
"	" "	"	Strangulated umbilical hernia.	"				1	Gangrene
"	Wien. Med. Blatt. 12, 1889.	Obtusowicz.	Prolapse intestine.	"			1		
"	Deut. Clin. (Sachs.) 32, '91.	Lumniczer.	Strangulated inguinrl hernia; gangrene.	"			1		
"	" "	Kaufman.	Closure artificial anus.	"			1		
"	" "	Park.	" " "	"			1		
"	" "	Bouily.	Hernia.	"			1		
"	" "	Frey.	Inguinal hernia, gangrene.	"			1		
"	" "	Martino.	" " "	"			1		
"	" "	"	" " "	"				1	
Feb. 12, 89	St.Peters.Med. Woch. 24. '89.	Wahl.	Carcinoma colon.	Colo-sigmoidostomy.	"Rub.tube	S	1		Koeberle Clamp.
Mch. 17, "	Gaz. Med. di Torino. '89.	Caponatto.	Closure artificial anus. strang. hernia	Entero-enterostomy.	"	S	1		Enterotome.
May 19, "	Arch. Gen. de Med. May, 91.	Chaput.	Closure artificial anus, stricture.	" colostomy.	"	S			
July 2, "	Med. Record, Nov. 29, '89.	Meyer.	Carcinoma colon.	Colo- "	" Lembert	S	1		

" 25, "	Rec.Med. de la Suisse Rom. 6, '90.	Comte.	" small intestine.	Jejunosjejunostomy.	"	S	1		D. 7½ mo. later. Extension
" 28, "	Arch. f. Klin. Chir. 42, '91.	Baracz.	Closure artificial anus, hernia.	Resection ileum.	" Czerny Lembert.	E		1	1 day. Peritonitis.
Aug. 7, "	Rec. Med. de la Suisse Rom. '89.	Comte.	Pyo-uretero-cæcal fistula.	Ileo-colostomy.		S		1	Failure. Artificial anus re-established.
Sept. 18, "	Arch. f. Klin. Chir. 42, '91.	Baracz.	Sarcoma, cæcum.	Resection ileum and cœcum.	" " "	E	1		1 day; collapse.
Nov. 8, "	T. Roy. Acad. Med. Ireland, '91.	Hayes.	Carcinoma transverse colon-	Colectomy.	" Lembert	E	1		D. after 9 mos. exhaustion.
Nov. 14, "	Arch. f. Klin. Chir, 41, .91.	Helferich.	Strangulated hernia.	Entero-enterostomy.	"	S		1	Immediate.
Jan. 23, '90.	Deut. Zeits. f. Chir. 34, '91.	Konig.	Stricture; tuberculous.	Enterectomy.	"	E	1		
Feb. 7, '90.	Gaz. hebd. Med. 27.'90	Thriar.	Closure artificial anus.	"	Gussenbauer.	E	1		
Feb. 24, '90	Daniels Tex. Med. Jour. Jan. '91.	Hadra.	Gunshot wound ileum.	"	Suture Czern. Lembert.	E		1	Few hrs; peritonitis.
Mch 28, '90.	Soc. de Chir. Par. '91.	Boiffin.	Stricture, ascending colon.	Entero-colostomy.	"	S	1		
Apr. 14, '90.	Centralblatt f. Chir. 31. 90.	Kredel.	Strangulated hernia.	" enterostomy.	"	S	1		
Apr. 23, '90.	Arch. f. Klin. Chir. 40, '90.	Konig.	Sarcoma, small intestine	Enterectomy.	"			1	After 24 hrs, rupture intest.
June 21, '90	Arch. gen. de Med. June, 91.	Chaput.	Closure artificial anus.	Entero-sigmoidostomy.		S	1		Forceps.
July 25, '90	Daniels Tex. Med. Jour. Jan. '91.	Hadra.	Ulceration ileum.	Enterectomy.	"	E	1		
Sept. 22, '90	Deut. Zeits f. Chir B. 34, 91.	Konig.	Stricture, tuberculous.	Colectomy.	"			1	3 days; collapse.
Oct. 8, '90.	Lancet, Mch. 21, 91.	Greig Smith.	Closure artificial anus.	Enterorrhaphy.	"	E	1		
Nov. 5, '90.	Hosp. Tidende, Apl. 15. 91.	Hansen.	Strangulated hernia.	Enterectomy.	"		1		
Dec. 11, '90.	Deut. Zeits. f. Chir. B. 34, 91.	Konig.	Stricture, tuberculous.	"	"			1	3 days; collapse.
Dec. 27, '90.	Rev. de Chir.	Llobet.	Sarcoma mesentery.	Resection small intest.	"	E	1		
'90.	Daniels Tex. Med. Jour., Jan., 91.	Hadra.	Strangulated hernia.	Entero-enterostomy.	" Czerny-Lembert.	S		1	6 hrs ; sepsis.
'90.	Centralblatt. f. Chir. 33. 90.	Salzwedel.	" "	" "	"	S	1		(Closure of opening). failure artificial anus established
Jan. 19, '91.	Arch. gen. de Med. June. 91.	Reclus & Chaput.	Closure artificial anus; Tuberculous cæcum.	Enterorrhaphy.	"	S		1	After 2 days; tuberculosis.
Feb. 2, '91.	Wien. Med. Woch, 39, 92	Schramm.	Gangrene, mesentery.	Enterectomy.	"Czerny.		1		
Feb. 20, '91.	Lancet, Feb. 6 93.	MacCormac.	Closure, artificial anus. Carcinoma.	"	" Lembert	E	1		Subsequent operation necessary; tried Senn's rings
Mch. 6, '91.	Nord. Med. Arkiv, 23, 91.	Studsgaard.	Traumatic stricture of ileum.	Resection small intestine.	" Lembert	E	1		
May 28, '91.	Wien. Klin. Woch. 53, 91	Frank.	Carcinoma æcum.	Ileo-colostomy.	"	E	1		
June 18, '91.	Wien. Med Woch. 39. 92	Cchramm.	Gangrene mesentery.	Enterectomy.	"		1		
July 16, '91.	Bull. et Mem. Soc. de Chir. Nov. 91.	Le Dentu.	Fibro-myoma of colon.	Colectomy.	" Lembert	E	1		
July 23, '91	Wien. Klin. Woch.	Frank.	Tuberculosis cæcum.	Resection.	"		1		

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Position.	Recov.	Death.	RESULT. Cause of death	REMARKS.
Apr. 1889.	Med. News, June 1, '89.	McBurney.	Carcinoma pylorus stenosis.	Gastro-enterostomy.	Abbe. Catgut rings.	S		1		After 12 hrs. Inanition.
Sept. 18, '89	Internal. Jour. Surg. Nov. '89.	Carson.	" "	" "	Brokaw. seg. rub rings.	S	1			
Dec. 5, '89.	Med. News, Dec. 14, '89.	Weir.	Construction " probably cicatricial.	" "	Abbe catgut rings.	S	1			Probable.
Jan. '90.	Med, Rec. Feb. 22, '90.	"	Stenosis "	" "	" "	S	1			
Mch. 29. '90	Med. News, May 17, '90,	Tuholske	" " Epitheliomia.	" "	Brokaw, seg. rub. rings.	S		1		After 26 hrs; exhaustion.
Apr. 10, '90.	Med. Rec.. Apr. 19, '90	Bull.	Carcinoma, stom. pylorus, pylorectomy.	" "	Abbe catgut ring.	S	1			First successful Am. oper. for cancer stomach.
Apr. 16, '90.	Med. Rec., Aug. 23, '80	Polk.	Carcinoma pylorus sten.	" "	" "	S		1	5 days.	
Aug. 10 '90	Med. Rec. Jan. 10, 91.	Bull.	" stomach.	" "	" "	S		1	7 days, faulty sut. of duod.	
Oct. 7, '90.	Med. Rec. Jan. 10, '91	"	" "	" "	" "	S		1	15 hrs; peritonitis.	
'90	" "	Weir.	Supposed fibrous stricture of pylorus.	" "	" "	S	1			
Sept. 29, '91.	Cleveland Med. Gaz., Feb., '92.	Scott.	Inflammatory stenosis of pylorus.	" "	" "	S	1			
Jan. 12, '91	Jour, Am. Med, Ass'n May 16, 91.	McGraw.	Carcinoma pylorus.	" "	Elastic lig. & Lembert.	S		1	16 days; exhaustion.	
Mch 5, '92	Montreal Med. Jour. May, '92.	Bell.	" stomach.	" "	Abbe catgut rings.	S		1	20 days; peritonitis.	
'93.	Unpublished.	Goldspohn.	Cancer pylorus.	Gastro-enterostomy.	Robinson seg. rub. plates.	S	1			
'93.	"	Lloyd.	" "	" "	" "	S		1	Progressive peritonitis.	
Oct., '93.	Unpublished.	Buecking.	" "	" "	Murphy but.	S	1			Time of op. 16 min. patient sub. died of exh., aut. showed perfect union.
Sept, '92.	"	Murphy.	" "	" "	" "	S	1			
May, '93.	"	Wiener.	" "	" "	" "	S				4 days; continued hem. from can.; perfeet union.
Aug. 9, '92.	"	Murphy.	" "	" "	" "	S	1			7 min.
June 7, '94.	"	Logan.	" "	" "	" "	S	1			
Dec. 26, '92.	Pac. M. J., Apl., '93.	Barbat.	" "	" "	" "	S	1			Autopsy 46 days; perfect union: no contraction.
July 25. '92.	Unpublished.	Ferguson.	" "	Pyrolectomy and gastro duodenostomy.	" "	E-S	1			Time 80 min. Sept. 3, pat good health,
Mar. '93.	"	Schede.	" "	Gastro-enterostomy.	" "	S		1	Exh.; perfect approxima.	
" "	"	"	" "	" "	" "	S	1			
Feb. 2, '94	"	Ferrier M.	" "	" "	" "	S		1	Exh. 18 hrs.; perfect union.	

OPERATIONS ON THE INTESTINES WITH MURPHY BUTTON.

Date.	Publication.	Operator.	Diagnosis.	Operation.	Method.	Position.	Recov.	Death.	Remarks.
Dec. 8 '92.	Unpublished.	Walker.	Strangulated hernia.	Resection intest. 4 in.	Murphy button.	E	1		First end-to-end approximation by button Oct. 21. '93. Pat. perfect health.
Apr. 3 '93.	Unpublished.	E.W Andrews	Femoral hernia, gang.	" " 18 in.	" "	E	1		Nov. 21 " " "
June 5 "	"	Murphy.	Intest. strangulation.	" " 2 in.	" "	E	1		Time for approx. 5½ min.
July 10 "	"	Murphy.	Umbilical hernia gangrene peritonitis.	" " 5 in.	" "	E		1	48 hours continued peritonitis. Perfect approx.
Oct. 17 "	"	Ruth.	Tumor-cœcum.	" cæcum, colon 2 in.	" "	E	1		(Nov. 15. excellent health).
Nov. 12 "	"	Bouffleur.	Femoral hernia, 5 days obstruction.	" intestine 4 in,	" "	E	1		Obstruct 6 mo. child 6 yrs. Time 4½ minutes.
Mch. 25 "	Med. & Surg. Reporter, Sept. 9, '93.	Sutton.	Closure artificial anus without resection.	Lateral anastomosis.	" "	S	1		Aug. 20, excellent health.
Sept. 10 "	Unpublished.	Murphy.	Fæcal fistula.	" " with't resec.	" "	S	1		Time 6 min. Oct. 24. Well. Fistula closed.
"	"	Rogers.	Ing. her. Gang. Res. 10 in	Resection intestine.	" "	E	1		Button passed 7th day.
Sept. 23 "	"	Murphy.	Fæcal fistula.	Lateral anastomosis,	" "	E	1		Time 7 minutes.
July, "	"	Mc Call.	" " gangrene.	" "	" "	S	1		
Oct. "	"	Marcy.	Cancer rectum.	Resection rectum, 4 in.	" "	E	1		Discharged 20th day.
Nov. 16 "	Jour. Am. Med. Association Feb 10 '94.	Cordier A. H.	Ileus-Cicatricial band.	Entero-enterostomy.	" "	S	1		
July 8 "	Unpublished,	Bacon.	Stricture rectum.	Bacon's op'n on rectum.	" "	E	1		May. 31. Good calibre: neither pain nor discharge. Rectum soft and pliable.
Jan. 14 "	"	Murphy.	Annular stricture rect.	Application of button.	" "	E	1		
Sept. "	"	Beck, Carl. Chicago.	Carcinoma of stomach, pylorus.	Maydl's operation.	" "	S-S	1		Died subseq'y of cancer; Postmortem; perfect union where button was used, no perit'l adhesion,
Dec. 11 "	"	Ferguson.	Cancer cæcum	Resection cæcum.	" "	E	1		
Feb. "	"	Beck, Carl.	Fæcal fistula.	Duod-jejunostomy	" "	S-S	1		Button passed 20th day.
Jan. 14 94.	"	Ferguson.	" "	Resection of ileum.	" "	E-S	1		" " 16th "
Jan. 30 93	Annals of Surg. June, '93.	Keen W. W.	Carcinoma of hepatic flexure of colon.	Ileo-colostomy.	" "	E-S	1		De'th 47 days later; ulcer of ascend. colon; perforat.
Feb. 3 94.	Unpublished.	Price Joseph.	Sig'd inv'd in malig. tu.	Resect. 6 in.; sigmoid	" "	E	1		Button passed 9th day.
Feb. 24 94.	"	Meyer Willy.	Carcinoma of pylorus.	Pylorectomy.	" "	E-S	1		Button voided 21st day.
Mch. 3 "	"	Meyer Willy.	Ileum inv'd in sarcoma.	Resection 15 in. ileum.	" "	E	1		
Jan' 2 "	"	Dodge W. T.	Fæcal fist., small intest.	" 3 " ileum.	" "	E	1		" " 8th
Feb. 16 "	"	" "	" " large intest.	" 3 " colon,	" "	E	1		" passed "
May "	"	" "	Ileum adh'd to ov. cyst.	" of ileum.	" "	E	1		Pat. died 11 days later from complication; union perfect: but. found in rect.
Jan. 15 "	Lancet, Apr. 21, '94.	Lane Arbuth.	Sar. of meso. sigmoid.	" 12 in. of colon.	" "	E	1		Button voided on 8th day.
May 9 "	Unpublished.	Bryant Joseph D.	Fæcal fistula (had 5 previous operations.	Ileo-colostomy.	" "	S-S	1		Oblong but. used for 1st time; passed on 15th day.
May 14 "	"	Shrady G. F.	Fæcal fistula.	Lateral approximation.	" "	S-S	1		
Feb. 12 "	"	McBurney C.	Tumor of cæcum.	Ileo-colostomy.	" "				Infection occurred; see details in history.

OPERATIONS ON THE INTESTINES WITH MURPHY BUTTON.

Date.	Publication.	Operator.	Diagnosis.	Operation.	Method.	Position.	Recov.	Death.	Remarks.
Apr. 27 "	"	Abbe.	Obs. of colon (malig.)	Lateral anastomosis.	" "	S-S	1		Growth to be resected.
Dec. 12 93.	"	McBurney & Murphy.	Fæcal fistula.	Lateral approximation.	" "	S-S	1		Button passed on 9th day through fistula
Apr. 1 94.	"	Davis.	Adherent ovarian cyst.	Entero-enterostomy.	" "	S-S	1		
Feb. 28 "	"	McLaren	Strangulated hernia.	" "	" "	E	1		Six in. of sigmoid removed.

ENTERO-ENTEROSTOMY BY SUTURE.

Date.	Publication.	Operator.	Diagnosis.	Operation.	Method.	Position.	Recov.	Death.	Remarks.
June 5 '88.	N. Y. Med. Jour, Mch. 2, '89.	Mc Cosh.	Strangulatcd Hernia; gangrene.	Entero-enterostomy.	Suture.	S	1		
Oct. 30 "	Brit. Med. Jour. Mch. 2 '89.	Franks.	Carcinoma, transv. colon.	Colo-colostomy.	" Gely.	E		1	
"	M. S. Reporter, LIX '88.	Agnew.		Ileo-ileostomy.	" Czerny-Lembert	S		1	1 day: sutures gave way,
"	Med. Chir. Trans. .89.	Inversen.	Tumor, sigm. flexure.	Colo-colostomy.	"	S	1		Intestine verical fistula.
Aug. 3 '89.	Lancet, Feb. 14, '91.	Jessett.	Org'c dis., cæcum obst.	Ileo-colostomy.	"	E-S		1	13 days; exhaustion.
Jan. 4 '90.	Dublin Jour. Med Sci., June, .93.	Franks.	Valvulus, small intestine- Gangrene.	Entero-enterostomy.	" Gely.	E		1	2 days; obstruction.
Jan. 17 "	Am, Jour. Med. Sci., May, .93.	McIntosh.	Closure artificial anus; adeno-sarcoma.	Colo-colostomy.	"	E	1		
Jan. 2 89.	Brit. Med. Jour. Nov. 16, '89.	Sinclair.	Closure artificial anus; hernia.	Ileo-ileostomy.	"	S	1		
Dec. 12 90.	Verh. d. deutsch Gesellschaf chir. '92	Braun.	Closure fæcal fistula; hernia,	Entero-enterostomy.	"	S	1		
" 17 "	" " "	"	Carc. small intest. obst.	" "	"	E		1	3 hrs: shock,
Jan. 15 91.	" " "	"	Clos. fæcal fistu., hernia.	" "	"	S		1	3 days; gangrene; peritonit.
Feb. 18 "	" " "	"	" " "	" "	"	S		1	4 days. "
May 5 "	" " "	"	Ileus.	" "	"	S		1	Immediate collapse.
" 13 "	Med. Red., Apl 2, '92.	Abbe.	Carcinoma signoid flex.	" "	"	S	1		
" 29 "	" " "	"	Closure fæcal fistula.	" "	"	S	1		

July 31 "	Laucet, Jan. 9, '92.	Barker.	Intussusception.	" "	" Barker.	S		1	10 hrs; gangrenous infection
Aug. 11 "	Deut. Med, Woch., June 8, .92.	Bier.	Closure artificial anus.	" "	" Lembert	S		1	6 days; perforative periton.
" 15 "	Lancet, Jan. 9, '92.	Barker.	Intussusception.	" "	" Barker.	S		1	14 hrs.
Sept. 23 "	Dublin Jour. Med. Sci., June, '93.	Franks.	Hernia, gangrene.	" " and enterorrhaphy.	" Gely.	E	1		
Oct. 8 "	Verh. d. deutsch gesellsch f. chir, '92.	Brann.	Closure of fæcal fistula, hernia.	" Enterostomy	"	E	1		
Nov. 3 "	Gac. Med. de Mexico, Dec. 15, '92.	Zarraga.	Closure of artificial anus penetrating wound.	" "	"	S	1		
Nov. 14 "	Med. Rec. Apr. 9, '92.	Weir.	Strangulation hernia.	Ileo-colostomy.	" Lembert	S	1		
Dec. 28 "	" " " 2, "	Abbe.	" "	Enterectomy.	"	S	1		
" 12 "	Verh. d. deutsch, gesellsch f. chir., '92.	Braun.	Tumor of mesentery.	Entero-enterostomy.	"	S		1	Few hrs; shock.
Jan. 13 92.	" " " "	"	Ileo-colic invagination.	Ileo-colostomy.	"	E	1		
Feb. 13 "	" " " "	"	Ileus.	Entero-enterostomy.	"	E	1		
Aug. 15 91.	St. Louis. Courier Med. Apr. '92.	Young.	Trauma, colon.	Colectomy.	"	E	1		
" 22 "	Wien. Med. Presse., 12, '92.	Rosenthal.	Strangulated hernia.	Resection.	"Lembert		1		
Sept. 6 "	Deut. Zeit. f. Chir., B. 34.	Konig.	Stricture, tuberculous.	Colectomy.	"			1	Five days; collapse.
Oct. 1 "	Bost. M. &. S. Jour., Mch. 3, '92.	Richardson.	Closure artificial anus, hernia.	Enterectomy.	" "	E	1		
Dec. 1 "	Jr. Am. Surg. Assoc.	Mc Cann.	Sarcoma, mesentery.	" "	"	E	1		
" 14 "	Deut. Zeit. f. Chir. B. 34.	Konig.	Stricture tuberculous.	" "	"		1		
"	Internat. Clin. 3. 91.	Carmalt.	Cancer cæcum.	" "	" "	E	1		
Jan. 9 92.	Bull. et. Mem. Soc. de Chir., Mch., '92.	Berger.	Closure artificial anus.	Enterorrhaphy.	" "	E	1		
Mch. 20 "	Wien. Klin. Woch.; 27, '92.	Frank.	Tuberculosis, cæcum.	Resection.	" "		1		
" 27 "	N. Y. Med. Jour. Sept. 10, 92.	Hartley.	Carcinoma, intussusception.	" "	" Maunsell.	E	1		
June 8 "	Va. Med. Monthly. Nov. '92.	Earle.	Closure artificial anus.	Colectomy.	" Halsted.		1		
	N. Y. Med. Jour., Sept. '92.	Kammerer.			" Abbe.	S	1		No contraction of opening. death 4 mos. later,
	Lyon Medicae.	Jaboulay.			Gussenbauer.	E	1		Artif. anus.
	" "	"			"	E	1		
	" "	"			"	E	1		
	Semaine Med. Jan. 16, '92	Larabrie.			"Lembert.	S	1		No resection.
	Gaz. deg. Ospit 89, '90.	Mazzucchelli.			" Czerny	E	1		
	Inter. Klin. Rund., May, 29. '92.	Hofmokel.	Intussusception.	Colo-colostomy.	"	E		1	
Dec. 18 '90.		Lukowicz.	Gangrene.	Resection. 5½ in.	" Czy. Lem.	E	1		Op. 3 hours.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Position.	Recov.	Death.	REMARKS.
Mch. 22 '89.	Med. News, June 1 89,	Abbe.	Closure of fæcal fistula.	Enterectomy.	Catgut rings.	S	1		
July 16 "	N. Y. Med. Jour. Sept. 14 '89.	Davis.	Intestinal obstruction.	Ileo-colostomy.	" mats.	S		1	After 14 hours, exhaustion.
Dec. 22 "	Med. Rec Apr. 2, 93.	Abbe,	Malig. stenosis, ileum.	Ileo-ileostomy.	" rings.	S		1	" 6 " "
90.	N. Y. Med. Jour. Apr. 19, '90.	Manley.	Strangulated hernia.	Entero-enterostomy.	" "	S		1	
Sept. 11 "	N. Y. Med. Rec. Dec. 20, '90.	Richmond.	Closure artificial anus.	Ileo-colostomy.	" "	S		1	" 40 " shock; peritonitis.
June 11 91.	Guy's Hosp. Rep. '91.	Davies-Colley	" " anus; Hernia	Ileo-colic implantation.	Rub. r. & sut.	E-S	1		Resection of prolap. bowel.
Nov. 30 "	Maryland Med. Jour. July 2, '92.	Ashton.	Malignant stricture, ileum.	Ileo-ileostomy.	Rubber rings. Ashton.	S	1		
	Lancet, Sept 24, '92.	Milton.	Closure artificial anus.	Enterorrhaphy.	Gelatine tube.	E	1		
92.	Brit. Med. Jour., May 21, '92.	Paul.	Strangulated Hernia.	Enterectomy.	Paul's bone tube.	E	1		
93.	Unpublished.	Martin.	Stricture of intestine.	Ileo-colostomy.	R's rub. plat's	S		1	Pre-existing peritonitis 10 hours after operation.
Nov. 14 88.	N. Y. Med. Jour., Mch. 23, '89.	Abbe.	Closure artificial anus.	Colo-colostomy.	Bone plates.	S	1		
Oct. 9 89.	Jour. Am, Med. Association, June 14 '89.	Senn.	Carcinoma cæcum.	Ileo-colostomy.	" "	S	1		
Nov. 14 "	" " " "		" ileo-cæcal valve.	"	" "	S		1	After 6 days, peritonitis.
July 18 91.	N. W. Lancet, May 15, '93.	Graff.	Strangulated femoral hernia.	Entero-enterostomy.	" "	S	1		
Aug. 26 "	" " " "		" " "	"	" "	S	1		Necr'c bl't fast'd in lum'n b
Sept. 14 "	" " " "		" " "	"	" "	S		1	After 4 days; faulty operat-
Aug. 7 "	Brit. Med. Jour. Apr. 22, '93.	Morison.	Malignant stricture. Ileo-Cæcal valve.	Ileo-colostomy.	" "	S	1		Death 10 weeks later. Bronchitis.
92.	" " "	Robson.	Closure artificial anus.	" "	Rob. b'n bob.	S	1		
"	Annl.Surg.Feb. 22 '93,	Currier.	Inj'g ile. dur'g ovarioto.	Ileo-ileostomy.	Potato plates.	S		1	After few hours, shock.
Jan, 24 93.	Lancet. Mch. 25, '93.	Lowson.	Malig. dis. ascend. colon.	Ileo-colostomy.	Bone plates.	S	1		
Aug. 17 89.	N. Y. Med. Jour. Dec. 20, '90.	Russell.	Stenosis; small intestine.	Jejuno-ileostomy.	Bone plates,	S	1		
Dec. 27 90.	Bost. Med. & Surg. Jour. Nov. 5, '91.	Homans.	Obstruction; invagination.	Entero-enterostomy,	" "	S	1		
Aug. 24 91.	Lancet Apr. 16, '92.	Littlewood.	Car. hep't flex. of colon.	Ileo-sigmoidostomy.	" "	S	1		
Nov. 26 91.	Lancet, May 7, "	Atkinson.	Acute intest'l obstruct.	Entero-colostomy.	" "	S	1		Short circuit.
Feb. 21 92.	Brit. Med. Jour. Apr. 30, '92.	Wright.	Closure artificial anus; hernia,	Entero-enterostomy.	" "	S	1		
	Brit. Med. Jour. Apr. 30, '92.	Barling.			Bone plates.	S		1	After 6 days, gangrene.

GASTRO-ENTEROSTOMY BY MECHANICAL MEANS.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Position.	Recov.	Death.	RESULT. Cause of death	REMARKS.
Dec. 16, '88.	Med. Rec. Nov. 14 '91.	Senn,	Car. pyl. & head of panc.	Gastro-enterostomy.	Senn's plates.	S		1		After 6 days, perf. periton.
" "	Med. Chir. Trans. '89.	Ransohoff.	" "	" "	" "	S	1			
" "	Med. Rec. Nov. 14 '91.	Senn.	" "	" "	" "	S		1		Death 5 days later, maras.
" "	" " " " " "	"	" "	" "	" "	S	1			" 14 weeks "
" "	" " " " " "	"	" "	" "	" "	S	1			" 3 " "
" "	" " " " " "	"	" "	" "	" "	S	1			" 4 months " maras.
Mch. 31. 89.	" " " " "	"	" " & head of panc.	" "	" "	S		1		After two hours, shock.
Apr. 10, "	" " " " "	"	" "	" "	" "	S	1			De'th 14 days, hæmorrhage,
Sept. 20, "	Brit. Med. Jour. Nov. 16, '89.	Clarke.	" "	" "	" "	S	1			carcinoma.
Dec. 16, "	Brit. Med. Jour. Feb. 8. '90.	Stansfield.	Pyloric stenosis, malignant.	" "	" "	S	1			Death 4 mo. later, maras. opening entirely closed.
Aug. 22, "	Med. News, Feb. 1 '90.	Stamm.	Carcinoma, pylorus.	" "	" "	S	1			" 43 days later, maras
Jan. 31,'90.	Med. Rec. Nov. 14 '91.	Senn.	" "	" "	" "	S	1			" 20 mo. " "
Feb. 23, "	Lancet, July 12. '90.	Jessett.	" "	" "	" "	S	1			5 d'ys, exhaustion. op,g pat.
Mch. 22, "	Med. Rec. Nov. 14 '91.	Senn.	" "	" "	" "	S	1			11 d'ys lat'r, croup. pneu.
Apr. 30, "	Lancet, May 23, '91.	Robson.	" "	" "	" "	S	1			" 95 " exh. no autopsy.
" "	" " " 2, "	Jessett.	" "	" "	" "	S	1			
May 13, "	" Dec. 6, '90.	Clarke.	"Gast. trou." hæmat.	" "	" "	S	1			" 30 " no autopsy
" 31, "	" Oct. 11, '90.	Beatson,	Carcinoma, pylorus	" "	" "	S	1		1	Af'r 3 d'ys asth.
" "	Brit. Med. Jour. May 17, '90.	Robson.	"	" "	" "	S	1			
June 4, "	Lancet Oct. 11. '90.	Beatson.	"	" "	" "	S	1			After 4 w'ks, pneu.
"	Med. Rec. Nov. 14 '91.	Senn.	"	" "	" "	S		1		After 5 hours, shock.
"	Brit. Med. Jour. Feb. 8, 90.	Rawdon,	"	" "	" "	S	1			
"	Lancet, July 12, '90.	Jessett.	"	" "	" "	S	1			
Jan. 22, '91.	Med. Rec. Nov. 14, 91.	Senn.	Pyl. sten., Dilatation.	" "	" "	S	1			After 18 days, exhaustion.
Feb. 27, "	Lancet May 2, "	Bennett.	Pyl. sten., malignant?	" "	" "	S	1			
Mch. 30, "	Med. Rec. Nov. 14, "	Senn.	Carcinoma. pylorus.	" "	" "	S	1			" 3 months, marasmus.
May 22, "	Lancet, June 11, "	Larken.	Malig. dis. of stomach.	" "	" "	S	1			Stenosis Sub't jejunost'y.
July 10, "	Brit. Med. Jour. Jan. 9, '92.	Barker.	Carcinoma, stomach.	" "	" "	S	1			Death 46 days, pneumonia.
Aug. 4, "	Lancet, Oct. 24, '91.	Jessett.	" pylorus.	Pyl'y & Gastro-enteros.	" "	S	1			
" 21, "	Med. Rec. Nov 14, '91.	Senn.	" "	Gastro-enterostomy.	" "	S	1		1	After 8 hrs; shock.
Sept. 8, "	Brit. Med. Jour. Apr. 22, '93.	Hume.	" "	" "	" "	S				" 9 weeks, extension of carcinoma.
" 10, "	Med. Chir. Trans. '92,	Robson.	" " sten.	" "	" "	S	1		1	After 3 days, exhaustion.
Oct. 26, "	Lancet, Mch. 28, '92.	Taylor.	" "	" "	" "	S				
Nov. 16, "	Brit. Med. Jour. Jan. 9, '92.	Renton	Tumor pylorus, malignant.	" "	" "	S	1			Disappearance of tumor.
Dec. "	Canad. Pract. Feb. 93.	Mc. Farlane.	Malignant dis., pylorus.	" "	" "	S	1			Opening entirely closed,
"	Lancet, May 2, '91.	Brown.	"Dysp. trou.," malig.	" "	" "	S		1		death, 6 months.

DATE.	PUBLICATION.	OPERATOR.	DIAGNOSIS.	OPERATION.	METHOD.	Position.	Recov.	Death.	RESULT. Cause of de'th	REMARKS.
Feb. 6, '92.	Glasg. Med. Jour. Dec. '92.	Renton.	Chronic dyspepsia. Dilatation.	Gastro-enterostomy.	Senn's plates.	S	1			Death 4 months
" 25, "	Wein. Klin. Woch. Nov. 3, '92.	Von Hacker.	Carcinoma, pylorus.	" " implantation.	Suture, and Senn's plates	E-S	1			
Mch. 19, "	Lancet, June 4, '92	Purcell.	" " stom.	" "	" "	S	1			
May 7, "	Centblt. f. Chir. June 11, '92.	Von Baracz.	" "	" "	Swed. turnip.	S	1			
Nov. 12, "	St. Petersberg. Med. Woch. May 15, '93.	Butz.	Stenosis, pylorus.	" "	" "	S	1			After 46 days, exhaustion.
Dec. 4, "	" " " " "	"	Cancer, pylorus.	" "	" "	S		1		" 72 hours "
" "	Lancet, Aug. 19, '93.	Moullin.	Chron. gast., non malig.	" "	Senn's plates	S				Pinhole op'. De'th 6 wks., exh.
"	Brit. Med. Jour. Apr. 1, '93.	Robson.	Carcinoma, pylorus.	" "	Robson's b. b.	S		1		After 6 days, exhaustion
"	Brit. Med. Jour. Apr. 22, '93.	Allingham.	Carcinoma pylorus, stenosis.	" "	" " " "	S	1			Death 3 weeks; exhaustion.
May 18, '90.	Brit. Med. Jour. Apr. 11, '91.	Clarke.	" "	" "	Senn's plates.	S	1			" 4 months; extension of disease.
Mch. 30 '91.	N. W. Lancet, May 15, '91.	Ramsey.	Tumor "	" "	" "	S	1			Death 3 weeks. Closure of opening.
Apr. 25. "	" " " " "		Cancer, "	" "	" "	S	1			
July 1, "	Austral. M. Gaz. July '92.	Hanleins,	Stenosis, pyl., non malig.	" "	" "	S	1			
" "	Med. News, Oct 1, '92.	Ruth.	Cancer, "	" "	" "	S	1			Death 7½ months. (Opening entirely closed.)
Nov. 11, "	Lv'pool M-C. Jour. 92.	Paul.	" "	" "	" "	S		1		After 12 hours, exhaustion.
			" " & stomach.	" "	" "	S	1			

DISCUSSION.

DR. W. W. KEEN.—Unfortunately I have not a very large experience to bring you in reference to the subject of intestinal anastomosis. In the quiet city from which I come our bowels probably do not get so much tangled up as they do in New York or Chicago. The number of cases I have to report are eight; four of these are cases of gastro-enterostomy, and the other four are cases of intestinal anastomosis proper. Of the four cases of gastro-enterostomy, one died at the end of twenty months; another of exhaustion at the end of the eleventh week; the third is living after fourteen months, and the fourth is living at the end of a year. Of the four cases of intestinal anastomosis, the first, in which I employed Murphy's button, died at the end of forty-seven days from a lesion independent of the operation—namely, perforation of the colon from a solitary ulcer. The second case died on the eleventh day after the operation from exhaustion. The third and the fourth died immediately after the operation—that is, within twenty-four hours; they were respectively cases of resection of the colon and cæcum and lateral anastomosis. I think now that it would have been wiser if in both of these cases I had been content with a lateral ileo-colostomy alone.

This constitutes my experience in this line of operative work, and the means I employed were as follows: In the first case of intestinal anastomosis, as already stated, I used Murphy's button; in the second, the segmented solid rubber ring of Ashton and Baldy, of Philadelphia; in all the other six, Abbe's catgut rings.

I would like to present here the specimen removed from the patient upon whom I operated by means of Murphy's button. You will observe, by looking at the outside of the bowel, that the anastomosis is simply perfect. You will also observe that the lumen of the bowel is only half an inch in diameter. I also show you the button which was used in this case, the diameter being one inch. Another part of the specimen consists of this circular portion of the bowel, which was necrosed between the two halves of the button. It will be seen, then, that the opening, which was originally one inch in diameter, had contracted in forty-seven days to half of its original size. How much more the opening would have contracted in seven months instead of seven weeks we cannot tell, but my fears are that it would have produced secondary intestinal obstruction at some time.

I take it, Mr. President, that in intestinal anastomosis there are four requisites, whatever the method employed may be. These are, first, simplicity of execution; second, speed; third, security; fourth, that after the operation the opening shall remain of sufficient calibre to obviate obstruction by cicatricial contraction. Dr. Murphy has presented the different methods by which these requisites may be secured—namely: first, by sutures alone; second, by sutures combined with mechanical means; third, by mechanical means alone. So far as mechanical appliances are concerned, the button is one of the most ingenious devices in the whole domain of surgery, and it merits our highest commendation. As regards speed it is far in advance of any other method at our command. As regards security, I think it is as secure as, and probably more so than, any other method. In the case in which I used it, I depended absolutely on the button alone without any sutures to reinforce it or to secure apposition of the muscular wall. The fatal objection to it, in my opinion, is that it will not secure that *sine qua non*—namely, a sufficient calibre of the bowel to allow of the passage of the intestinal contents. In making this criticism Dr. Murphy will understand that it is absolutely impersonal, and that it is solely made for the purpose of getting the best results, by whatever means possible, by whomsoever or wheresoever devised.

I do not know what will be the ultimate outcome of this matter regarding the narrowing of the lumen of the bowel after the use of the button. The cases that have been reported have not lived sufficiently long to enable us to reach a definite conclusion, and I do not think it is proved as yet that this means of union is going to give us the ideal result or the necessary result, which is one of continued ample patency of the calibre of the bowel. Although I am perfectly well aware that in New York the tendency has been toward discarding mechanical means entirely, I confess that for myself I am still a believer in mechanical means, and, above all, in that devised by your distinguished fellow-member, Dr. Abbe. I like it better than any other. The large surface that you have with Senn's bone plates, and, in addition to that, the comparatively small opening, does not commend itself to me as much as the sufficiently large (and yet comparatively small) surface of apposition of the bowels that you have with Abbe's rings; at the same time you can have as large an opening as you like. I feel tolerably certain

that the very large opening with sutures only, lately proposed by Dr. Abbe, is needlessly large. One of my patients upon whom I performed a gastro-enterostomy twenty months ago died last month. In that case I made the opening two inches long and the patient lived in comfort for twenty months. I could not obtain a post-mortem examination, and therefore cannot give you the final result as to the opening. Still, if you bear in mind the fact that in this case the operation was done for cancer of the pylorus, and that at the end of twenty months there were no symptoms of contraction of the cicatrix, I am sure you will agree with me that the man lived out his allotted life.

One objection that has been made to Dr. Murphy's button is that the necrosis of tissue would perhaps become more extensive than we desired. I do not think this is a valid objection. It seems to me quite certain, from our frequent experience with pressure necrosis, that this will not extend beyond the limits of the pressure of the two halves of the button. Moreover, actual experience with the button has shown that this is not a valid objection. It seems to me that the objection I have given of the narrowing of the opening is the essential one. I cannot admit the correctness of the explanation that has been given of the narrowing of the opening in the case I reported and of which I showed the specimen. If I understood it rightly, the explanation was that there was a subsidence of the œdema of the surrounding tissues and a consequent and natural narrowing of the bowel. In that case there was no reason for œdema. The bowel was cut away in a perfectly healthy portion, and was united to the colon in a location where that organ also was not œdematous, but perfectly healthy. So far as I can see, there was no reason whatever for any œdema of the tissues, save that due to the constriction and necrosis contemplated by the use of the button, and therefore the opening must have contracted by the natural cicatricial contraction that we find in all these operations.

In one class of cases I think Dr. Murphy's device will prove superior to all other means—namely, in cholecyst-enterostomy, which is one of the most difficult operations done in the abdomen. There we have only fluid contents which must pass through the opening. It is true there may be a formation of gall stones, but if the calibre of the button is sufficiently large these could pass through without any difficulty. I believe, therefore, that in chole-

cystenterostomy this device is superior to all others, with one reservation, which I make rather theoretically, and that is, that I should not like to have any button in my intestines. I should not like to have anything in my intestines that is likely to become arrested at the ileo-cæcal valve. However, I do not believe that any theoretical objection should be allowed to stand in the way of absolute facts. Dr. Murphy has cited sixteen cases and two hundred and sixty experimental instances in which no obstruction took place following the use of the button; still, it is by no means certain as yet that in human beings obstruction may not take place. Gall stones, much smaller than the buttons, especially some of the large ones that have been shown here, have been the means of causing obstruction.

In conclusion, I desire to thank you for your patience in listening to me, and, in view of the lateness of the hour and the fact that other speakers are to follow, I shall not take up any more of your time. I am very much interested in Dr. Murphy's button. It is novel and ingenious, and in many respects is the best method of securing intestinal anastomosis. At the same time, in all honesty, which we owe to our patients, to ourselves, and to science, I cannot yet regard it in all respects as the best and safest method.

DR. R. F. WEIR.—Dr. Keen has expressed himself so happily in his discussion of Dr. Murphy's paper, and has covered the ground so thoroughly, that I shall only add a word or two. I feel, with him, that in connection with operations on the gall bladder and intestines the use of the button is especially adapted. It is possible in such a case that the button will become lodged in the gall bladder and fail to pass into the intestines, but such an instance has not yet occurred.

In connection with gastro-enterostomy, I think the button, or any plate or ring, can be entirely done away with. I believe the consensus of opinion of the surgeons in New York is now properly against the use of mechanical devices in this operation. We believe from our own experience that such mechanical contrivances do not afford sufficient room, leaving out other serious objections, for obtaining a satisfactory opening between the stomach and intestine. We require at least two and a half and possibly three inches incisions for this purpose; and while I have been able to accomplish this suturing, with the aid

of Abbe's rings, in forty-five minutes from the time of entering the abdomen to the closing of the abdominal wound, in another case, without any mechanical help of the kind, the operation was as speedily accomplished.

In respect to the lateral operation on the intestines—lateral anastomosis—I do not feel that we are warranted in the use of plugs of the size required, lest, as Dr. Keen stated, they should fail to pass the ileo-cæcal valve. It is necessary, in my opinion, to make the opening fully as long as Dr. Abbe has advocated—namely, from three and a quarter to four inches—in order to obviate cicatricial contraction.

I desire to present here a man on whom was made a lateral anastomosis by long incisions. His belly bears the marks of sundry surgical procedures. In May, 1891, a herniotomy was performed on him by the house surgeon of the New York Hospital. Some months subsequently a tumor appeared in the abdomen; an exploratory incision was made by a colleague, and a large growth found, incapable of being removed, and threatening stenosis of the transverse colon, which was invaded by the growth. As the symptoms of obstruction were not very marked, though progressing, the growth was not excised, but an incision was made over the cæcum and the cæcum stitched to the abdominal wall, preparatory to a future opening, should this prove necessary. He then came under my care, and, his symptoms of obstruction becoming more severe, I explained to him the advantages of intestinal anastomosis over an artificial anus, and he chose the former operation. Lateral union was made with an opening three and a half inches long. Though at the exploratory operation a diagnosis of malignant growth had been made, this was in time abandoned, as the tumor began to diminish in size. The indurated mass was probably due to a slowly progressing inflammation of the omentum, such as occasionally follows ligation of that tissue when so dealt with in the radical cure of a hernia. Two years have elapsed since the operation of anastomosis. I cannot determine whether the opening made in this case has diminished in size. The man, however, within the past four months has had two attacks of temporary obstruction of the bowels; this points rather strongly toward narrowing of the lateral opening, or they may be due to some adhesions.

I also take this opportunity of showing a specimen in a

case of anastomosis between the stomach and intestine in which Abbe's rings were employed. The inner ring became displaced by twisting, and a hæmorrhage ensued which caused the death of the patient. I will also show a Murphy button *in situ* employed in a case that terminated unfortunately. The case was one of anastomosis between the gall bladder and duodenum for carcinoma of the latter structure. Its details will be found in a future number of the *Medical Record.*

I cannot close without reverting to one other point in connection with this subject. I do not believe that lateral anastomosis can be better accomplished by means of these buttons than by the ordinary method of suturing. The mortality of lateral anastomosis is comparatively low. The too few cases where the button has been used do not present any better mortality. The mortality of end-to-end anastomosis, by the use of the button, has been brought down, I believe, to thirteen or fourteen per cent. This does not exceed very materially the figures we get after end-to-end suturing. There is always an element of uncertainty about statistics. It is better to study the statistics of one surgeon or one hospital. Kocher and others have reported a very low mortality rate from the end-to-end suturing operation, and Czerny reports eighteen cases with only two deaths. He did the gastro-enterostomy operation within thirty minutes, and does not take much longer, in many instances, to do his resections. As to the question of stenosis that follows end-to-end union of resected intestine, it is somewhat of a surgical bugbear. Of twenty-six cases reported by Czerny, nine were alive two years after the operation. It is to be inferred from this that there is no marked stenosis in these cases.

Notwithstanding these various objections I have ventured to make, I feel much indebted, in common with my surgical colleagues, to Dr. Murphy for his able résumé of this entire subject of intestinal anastomosis.

DR. R. ABBE.—I feel with Dr. Keen that we are somewhat handicapped in our discussion of this subject by the sluggish condition of the Eastern bowels. We do not meet with so many cases demanding surgical interference. I cannot but agree with Dr. Weir that statistics are often untrustworthy; that we cannot rely upon them, but must take our practical experience and our feelings and impressions as guides. I was struck partic-

ularly by the statistics given by Dr. Murphy: he stated that the rings were employed in five cases with five deaths. I have met with no such results here in New York. A large number of operations have been performed here in which the rings were employed with the happiest results. Some of these are on record, not all.

Various mechanical devices have been employed for the purpose of intestinal anastomosis, and according as the surgeon is dexterous in the use of one or the other he prefers it. Most of these devices have been abandoned, even the bone plates, on account of the disastrous record following their use in gastro-enterostomy. After all, we have left for the practical surgeon and for the student of surgery a method that always appeals to him—namely, the suture. Any surgeon dexterous with the use of the needle and thread has at hand a method which will always serve him in safely uniting intestinal surfaces. All it requires is a little acquaintance with the technique.

The statistics given by Dr. Murphy of lateral and end-to-end anastomosis also need revision. In lateral anastomosis I think it very important that a large opening should be made. We have to face a law of stenosis which is irrevocable. After the abdomen is closed it is beyond the surgeon's control to prevent stenosis. The importance of leaving a large opening was impressed on me in four striking cases of lateral anastomosis. In the first two, in one of which I employed Senn's plates and in the other my catgut rings, I made the opening one and one-half inches long; both of these died after six months, one of carcinoma and the other of tuberculosis. In one the autopsy showed that the opening had contracted to such an extent that the tip of the little finger could barely pass through; the other had contracted to about one-half inch. These patients required an occasional laxative to clear their bowels. Two and one-half years ago I did two operations of lateral anastomosis most satisfactorily by suture alone, making the openings four inches long. Those patients are entirely well to-day.

When it comes to the question of speed I grant that Murphy's button is brilliant in its device. There is nothing like it. It is apparently the climax of surgical ingenuity, and if it could be shown that end-to-end anastomosis of the small intestine by means of the button can be accomplished without the danger of obstructing the canal by the button itself, and, furthermore, without the

danger of subsequent cicatricial stenosis, then a wonderful advance will have been achieved in this field of surgery. This remains yet to be proven. As Dr. Keen said, gall stones of small size may at times obstruct the canal, and why not a button of this size? In cholecystenterostomy the button has deserved its laurels. It is well known, however, that gall stones of considerable size have ulcerated through into the intestine or stomach, followed by cicatrization and stenosis at the site of the ulceration, and it may yet be shown that the opening made by the button will also close. Another objection to the use of the button in cholecystenterostomy is that, when there is occlusion of the common duct by a gall stone, the use of the button ignores the cause of obstruction and leaves it permanently there. In such cases it seems more surgical to open the gall bladder and remove the stone, leaving a fistula for a time, which closes and leaves the patient permanently well. In this way we pursue a purely surgical scientific method, getting rid of the cause of the trouble. If there is cancerous obstruction of the duct, there is nothing so good as Murphy's button.

Statistics to-day are better than they were formerly, because we are gaining ground by clean surgical peritoneal work. Old statistics count for nothing. Almost any method will give you good results if you do clean work and avoid peritonitis. It can be done by the surgeon who is armed with a needle and fine silk thread, and these he will find everywhere. Linen thread boiled for twenty minutes will also answer the purpose. Then he is ready to undertake any case of intestinal anastomosis.

DR. B. F. CURTIS.—We all admire the ingenuity of the Murphy button, but, as has already been shown, there are objections to it. We all hope that in the future this ingenious contrivance will triumph over the possible danger of stenosis and intestinal obstruction.

I will only speak of one point in connection with the use of the button. The principal argument that has been advanced in favor of the introduction of mechanical aids in intestinal anastomosis is the saving of time. It seems to me that while the duration of the operation is a very important factor to its success, yet it is not always important. Intestinal operations are done for two classes of cases—namely, for acute cases, and for chronic obstruction wherein the question of time is of little importance.

In the latter cases the patient can be prepared for the operation by a course of laxatives, etc., for days or perhaps weeks beforehand. Such patients then can bear an ordinary surgical operation lasting for an hour or an hour and a half without any difficulty, and in such instances mechanical means have no advantage over suturing. For acute cases, however, it is different. Here we have to deal with a patient in collapse, exhausted by vomiting and by prolonged obstruction, due perhaps to the procrastination of the physician. In these cases the emergency can be met in another way besides the use of mechanical means, and that is by making an artificial anus above the seat of the obstruction. This occupies only a few minutes, and does not draw on the strength of the patient at all, and affords immediate relief. The fear that an artificial anus will be permanent is not well grounded. In a large majority of cases where this operation is done for acute obstruction the opening finally closes of its own accord, and in many instances the natural passage through the bowel is restored. Therefore, even in these desperate cases we need not resort to mechanical contrivances. At the same time we must congratulate Dr. Murphy upon this remarkable invention, and we all hope that his good success with it will continue. I would like to inquire how the pressure of the button affects the mesenteric junction when it is used in end-to-end anastomosis. I do not understand how the button produces an equal pressure on the entire circumference of the intestine where the included parts are not of equal thickness, for at the mesenteric junction there is so much more tissue in the grasp of the instrument than elsewhere.

DR. MURPHY (closing the discussion).—In reply to Dr. Curtis' question I would say that there is a small spring attached to the button, and this adapts itself to the thickness of the intestinal wall. It is therefore uniform in its pressure over the entire surface.

I desire to thank the gentlemen who discussed my paper for their candor. They have exactly the same feeling about the button that I had. The two questions that have often occurred to my mind in connection with this contrivance are, first, the obstruction that might occur from the button itself after it has become liberated; second, the secondary cicatricial contraction of the bowel. If we do an end-to-end approximation with the button, we have an opening larger than we could get by any other

method. With the Czerny-Lembert suture you cannot get so large an opening, on account of the invagination. On first making up my statistics I was much surprised to learn that the mortality from the end-to-end approximation was about one-half of that by the lateral operation. The end-to-end should therefore always be performed after resection. The statistics, I may state, were compiled for me by Dr. Stanton, of Chicago, who did not know for what purpose they were intended. If the opening left by the button is larger than that obtained by end-to-end anastomosis with suture, then we have gained a great deal. The damage done to the tissues by the button is less than that done by other methods, and hence less fibrous deposit will result. Contraction of the bowel is due to the fibrous accumulations, as in stenosis of the rectum. In the intestines we have to deal with tissues of similar histological composition, and when they are approximated in juxtaposition there is a restoration of their normal continuity. Thus I have been informed by the pathologists in their reports herewith submitted, and the slides are here for your inspection.

What is the danger of obstruction by the button itself? How many surgeons here have seen little children who swallowed a cent, or even a twenty-five-cent piece, and what did those surgeons tell the mothers of those children? They told them to go home and give no more thought to the matter. They did not mean to say that such a foreign body could not produce obstruction, but that in the great majority of cases they would safely pass through the intestinal canal. I believe these buttons will not produce obstruction unless there exists some previous pathological contraction. The experiments made on the ileo-cæcal valve justify this.

In reply to Dr. Keen I would say that Dr. Mayo did not state that in the case referred to there was œdema; he said that with intestinal obstruction there was distention of the bowels at the time of operation, and that the calibre of the opening did not subsequently contract more than the calibre of the bowel itself from its pre-operative distended condition to its normal contracted state.

Stated Meeting, December 21st, 1893.

THE BABCOCK TEST AS A MEANS OF DETERMINING THE COMMERCIAL AND NUTRITIVE VALUE OF MILK (BOVINE AND HUMAN).

By HENRY G. PIFFARD, M.D.

As every one knows, the ordinary methods of chemical analysis as applied to milk (gravimetric process), with a view to the determination of its richness in oil, are tedious and impracticable except at the hand of a skilled chemist. A simpler means was therefore a desideratum, and this has been supplied by Prof. S. M. Babcock, Ph.D., of the University of Wisconsin.

Babcock's method consists, first, in dissociating the oil globules or butter fat from the casein by means of sulphuric acid and heat; second, in collecting the butter fat in one mass by means of centrifugal action; and, third, in measuring the volume of the liquid fat in a suitable graduated tube. For the convenient carrying out of this process Dr. Babcock has devised a simple and inexpensive apparatus, consisting of a pipette, a graduated cylindrical glass, and a centrifugal holding a number of test bottles. Some sulphuric acid and a little hot water include the accessories. With these appliances an exceedingly accurate determination of the butter fat contained in a sample of milk can be made in about ten minutes.

The test bottle is of about two ounces capacity, with a narrow, graduated neck. The graduations run from one to ten, and each of the divisions corresponds to the percentage of butter fat contained in the sample of milk tested. The main divisions are further subdivided, so that per-

centages as small as 0.2 can be easily read off. The pipette has a single graduation marked 17.6 cubic centimetres, and the cylindrical graduate has also but a single mark at 17.5 cubic centimetres.

The centrifugal is a large tin drum with a cover, and inside there are supports for the test bottles. Outside there is a gearing by means of which the bottles can be whirled at great speed.

In order to test any given sample of milk, care should be taken that it is thoroughly shaken up, so that any cream that has risen by standing shall be evenly distributed through the mass. When this is done insert the point of the pipette into the milk and suck it up until it rises above the graduation. Place the finger over the upper end, and by admitting air slowly allow the milk to escape until it stands exactly at the 17.6 cubic centimetre mark. Then transfer the milk to the test bottle, blowing out the last drop.

Next fill the acid measure to the 17.5 cubic centimetre mark with sulphuric acid (sp. gr. 1.82), and add it slowly to the milk. In doing this it is well to slightly incline the bottle, so that the acid will run down along the side, rather than to drop it directly through the milk. When the acid is all in, shake the test bottle slowly and with a circular motion, so as to thoroughly mix the contents. This will be accompanied with the development of considerable heat, and the mixture will become black from carbonization of the milk sugar.

The test bottle is now placed in the centrifugal, and the other bottles, if not used for testing, should be nearly filled with water. Next place the cover on and turn the wheel at moderate speed for exactly five minutes; the bottles are then removed from the centrifugal, and hot water is added until the fluid rises nearly to the upper graduation on the neck. The test bottles are returned to the centrifugal and whirled for one minute. This completes the operation. The cover being removed, the bottles are taken out and are ready for examination.

The entire quantity of butter fat will be found collected in the neck of the bottle, and its proportion can be easily read off. For instance, if the upper line of the butter fat is at 8.6 and the lower at 3.1, it would indicate that the milk contained five and one-half per cent of fat—which figure, however, is higher than the milk from the majority of cows will test.

The Babcock method is now almost exclusively relied on to ascertain the commercial value of milk when bought or sold in large quantities for butter- and cheese-making.

Although the Babcock test gives a direct indication only of the proportion of butter fat contained in the milk, it is also relied on for the determination of the casein, because analysis has shown that this latter substance bears an almost constant ratio to the former—*i.e.*, the more fat the more casein, and *vice versa*.

It will thus be seen that by means of the simple procedures here shown we can determine with great ease the proportions of the chief nutrients (hydrocarbons and albuminoids) contained in any given sample of milk. So far as I am aware, however, no definite relationship of the milk sugar to the varying proportions of the other ingredients has been worked out, and, if I am correct, opens a useful field for investigation.

The utility of this invention of Prof. Babcock, in enabling managers of hospitals, asylums, and other public institutions to control the quality of milk furnished them, suggests itself at once.

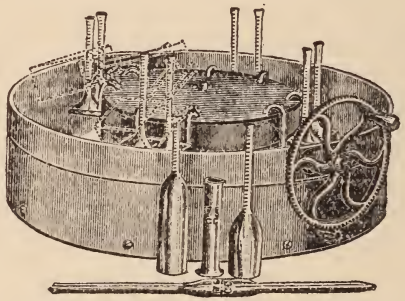
In private families, even, this test can be utilized. In my own domestic economy I had been paying ten cents a quart for the milk furnished by a well-known purveyor. A competing milkman offered to furnish "just as good" for seven cents. A few tests were enough to show that the ten-cent milk was the cheaper.

Some twenty years ago I made a good many analyses of human milk by the processes then in vogue, but, as will be understood, the time required for these examina-

tions was more than a practitioner could well afford to give. The Babcock machine, however, now makes this a comparatively easy matter, and I find that satisfactory examinations can be made of mothers' and wet-nurses' milk without any modification of the processes here described.

The Babcock test offers also a convenient means for the examination of some of the proprietary milk foods now so plentiful in the market.

Some months ago a sample distributer called at my office with a preparation that he declared was made only from the "best of pure country milk." After listening to



the fellow's story I suspected that the thing was a fake, and on submitting his preparation to the machine found that it contained less than two-tenths of one per cent of butter fat. In other words, the stuff was made from skim milk.

The Babcock centrifuge, however, can be applied to other uses besides milk-testing, as it may also be employed to obtain a prompt separation of urinary sediments. For this purpose I use bottles with a shorter and broader neck, and made, of course, without graduations. After two or three minutes' whirl the urinary sediment, crystals, casts, etc., will be found collected in the bottom of the bottle, ready for immediate microscopical examination, thus obviating the usual delay of twelve or twenty-four hours required for settling in the ordinary way. The import-

ance of this, in enabling a physician to arrive at an early diagnosis in a doubtful case of uræmia, will not be overlooked.

Finally, those who delight to delve into the diverting depths of the fascinating science of diatomology will find this centrifuge a substantial aid in their work.

Dr. Babcock, with great generosity, has given his invention to the world, and as a consequence a dozen or more firms are engaged in its manufacture, each, however, introducing some special modification of their own. The one here shown is known as Roe's pattern, and is made by the Creamery Package Manufacturing Company, of Chicago, but can also be obtained from Richards & Co., of this city. The price of the four-bottle machine, with appurtenances, is $8. Those arranged for a larger number of bottles cost a little more.

ASTHENOPIA NOT DEPENDENT UPON ERRORS OF REFRACTION OR INSUFFICIENCY OF THE OCULAR MUSCLES.

By THOMAS R. POOLEY, M.D.

Lord Macaulay is credited with saying: "I wish I was as sure of anything as some people are of everything." I may begin this paper by paraphrasing this remark, and saying that I wish I was as sure of anything as some—I might almost say all—ophthalmologists are that every case of asthenopia or painful vision is due to, or depends upon, underlying errors of refraction or insufficiency of the eye muscles.

It is a fact which cannot be denied, and which finds its support in the experience of all who concern themselves with the treatment and study of eye diseases, that the majority of cases of asthenopia do depend upon either one or the other, or both, of these causes. To maintain that all cases of painful vision, however, are due to such

defects is as manifestly absurd as to attribute different forms of nervous affection and headaches entirely to errors of refraction or muscular insufficiencies, or to assert on the other hand, as do some distinguished authorities, that asthenopia is never due to muscular insufficiency, but always to the error of refraction which exists when there is insufficiency of these muscles. It may be safely asserted that every person at some time or other in his life suffers from painful vision. Has everybody, therefore, an error of refraction? Probably not, although some would have us believe that such is the case, and even the distinguished Helmholtz denies the existence of a perfectly emmetropic eye. Admitting, therefore, the existence of errors of refraction and defects of the eye muscles as the prominent cause of asthenopia, I still maintain that there are other causes, some of which are pretty well understood, and others of which are more obscure. It will be the aim of this paper, therefore, to inquire into those conditions which bring about such results, and suggest, in default of absolute knowledge or proof, others which may have an important bearing on the subject.

In discussing this question it will be convenient to consider (1) local causes, or those which act directly upon the eye itself, and (2) those which act indirectly, first affecting more or less the entire system, and secondarily showing their effects in painful vision. It must be constantly borne in mind that the act of vision is only part and parcel of a series of phenomena, and that the integrity of the entire nervous system is necessary for the proper performance of their physiological functions in order that all may act harmoniously and painlessly. That inherent defects in the eye itself may hasten the occurrence of asthenopia, even where trivial errors of refraction exist, is self-evident; but it is equally true that even the healthy eye will refuse to withhold the expression of resentment if the act of vision be too greatly prolonged.

The first cause, then, of asthenopia which suggests itself is that due to over-use of the eye. We are all familiar with the typical asthenopia of the hypermetrope, in which the necessity for the expenditure of accommodation, even for distant vision, and much more in proportion for seeing near by, exists. At first near vision is distinct and the work goes on well, but after a while the object point, or whatever is looked at, grows indistinct and blurred as though enveloped in a slight haze. This is due to the fact that, owing to an overstrain of the accommodation, it gives way and the eye ceases to be properly focussed. A short period of rest, during which the eyes look at a distance or are kept closed, enables them to continue, but the same obscuration sets in again and compels another pause. These periods of enforced rest are more frequently repeated, and are of greater duration the longer the work is kept up. With them are associated pains in the eyes, and more especially pains in the forehead, and headaches. These symptoms make their appearance at first only after prolonged work—namely, toward evening—but afterward they come on earlier and earlier, until work has to be set aside after a short period of exertion. After quite a prolonged rest—for example, after the repose of Sunday, or upon laying aside work for several weeks—the symptoms probably disappear for several days in succession, but only to reappear in the old way and in a more aggravated fashion. They are dependent upon the exhaustion of the ciliary muscle, and are hence comprised under the name of "accommodative asthenopia," to distinguish them from muscular asthenopia and the other forms to be considered in this paper.

While the existence of hypermetropia lies therefore at the bottom of most cases, I maintain that, much more frequently than is usually supposed, near vision continued for many hours, day in and day out, is sufficient to bring about the same result in the rigidly emmetropic eye, thus exhausting the accommodative facility of the eye and

causing the same serious symptoms as those just delineated as occurring with the hypermetropic eye, which may be called "accommodative asthenopia," from over-use of the eye.

In eyes which have been thus used, or rather abused, the symptoms of asthenopia cannot be overcome by the use of glasses, although the use of feeble convex lenses may afford some slight relief; nor can it be overcome by mere periods of temporary rest; it becomes necessary to rest them for a long time, and then to return to using them gradually—at first for a short period—before the ciliary muscle regains its full vigor and will permit close application in the near without discomfort ensuing. It is not, however, my intention in this paper, nor will the time at my disposal allow me, to burden the hearers with a long list of cases, but only to insert an occasional one here and there in support of my position. The following illustrates such a case as I have just been describing, in a most practical and typical manner:

H. V. P., a young gentleman of about twenty years of age, who had had the misfortune to lose one eye in infancy, consulted me on account of painful vision in the other. The symptoms he enumerated were exactly those just gone over—indistinctness of vision, blurring of the sight after use of the eye, and frontal headache if he persisted in using it. He was a student of technology in the Stevens Institute, and, being exceedingly ambitious and industrious, he used his eye not only all day, but well into the night, until the symptoms of asthenopia had become so unbearable that he was obliged to desist from work and apply for help. Examination showed no error of refraction; he was unquestionably emmetropic. He had vision $\frac{20}{15}$. The loss of one eye, and consequently of binocular vision, put the question of muscular trouble at rest. An ophthalmoscopic examination revealed capillary hyperæmia of the optic disc such as may be seen in any aggravated case of asthenopia. Inasmuch as there was no inherent defect of the eye, but aggravated asthenopia nevertheless, the nature of the case was explained to him, and he was directed to use his eye less, only for a short time without resting, and only during the day. There

was, however, but slight improvement in his symptoms, and upon my writing to his father he was forced to give up all work for a time, and his eye then ceased to pain him. So soon as he returned to college and work, however, the old symptoms reappeared, and it became necessary for him to give up all idea of completing his studies. He is now engaged in a business where he uses his eye only moderately, but his studious tendencies are a constant incentive to excessive work, and as sure as he uses the eye too much he has a return of the pain and headache.

This is no doubt an extreme case, made more aggravated by the patient having only one eye, but I could enumerate many more in the same category where the patients had two eyes. This one, however, may be sufficient to prove what is well borne out in the writer's experience, and no doubt in that of other ophthalmologists as well.

That the source of illumination where the patient works may have a great deal to do in determining this is well shown in a recent article by Dr. C. J. McLeod, of Sydney,[1] on "Electric Light Asthenopia," in which he relates three cases in which, with other symptoms, there was asthenopia. There was no other cause or determining feature other than functional exhaustion of the retina from excessive use of the eyes in a room lighted by electricity. In all, the arc light was used, and the author says that there are no cases in evidence from the use of the incandescent light, which I believe is the one generally in use here.

A possible effect of toxic agents upon vision must not be lost sight of—for example, the mydriatic class of drugs, which affect too the accommodation, and the action of which when dropped in the eye is too well known to need even mention; but these drugs may act, too, when given internally, or even when used endermically. The writer has seen asthenopia resulting from the

[1] Australasian Medical Gazette, Sydney, 1893, xii., pp. 195–198.

internal administration of hyoscyamin and the use of a belladonna plaster worn for the relief of lumbago, and its consequent effect on the ciliary muscle, causing weakness of accommodation, with consequent inability to maintain protracted work in the near. Another example of this is seen in lead-poisoning, in which one of the first effects is observed in the ciliary muscle, causing various degrees of paresis of accommodation and consequent asthenopia. Later the optic nerve and retina become affected. The excessive use of tobacco and alcohol acts in this way, as well as by affecting the optic nerve and retina.

The next cause of asthenopia to be considered is that which is due to diseases of the nose. Dr. Noyes was, I believe, the first, in this country certainly, to call attention to the dependence of asthenopia upon catarrhal affections, which he did in 1867 in a paper read before the International Ophthalmological Congress; but he merely mentions them without defining the pathological conditions especially liable to affect the eye. On January 30th, 1886, Dr. E. Gruening read a very interesting and instructive paper before this Academy entitled "Reflex Ocular Symptoms in Nasal Affections," in which he gives the result of some clinical observations tending to show that certain groups of ocular symptoms may be looked upon as reflex neuroses, and furthermore that the source of the ocular disturbance may be found in certain alterations of the nasal structures. The relation between the nerves of the nose and those of the eye, as well as reflex ocular symptoms—for instance, lachrymation, conjunctival hyperæmia, and photophobia—may be readily demonstrated by touching certain parts of the Schneiderian membrane with a probe; and, *vice versa*, reflex nasal symptoms—sneezing—may be provoked by the effect of light on certain irritable eyes. In the cases observed by Gruening the ocular symptoms present were lachrymation, photophobia to ordinary light, redness of the eyes, and weakness of vision or asthenopia, and yet the exami-

nation of the eyes showed absolutely no abnormality. The refraction may be emmetropic, the vision normal, the muscular balance true, the conjunctiva healthy, the puncta lachrymalia favorably placed, and the nasal ducts permeable. The conclusive point in favor of such cases being due to affections of the nose is that, all other means failing to cure them, nasal treatment proved efficacious. Gruening enumerates five cases in which the nasal alterations were but slight and consisted in simple catarrhal affections of the Schneiderian membrane with insignificant swelling of the corpora cavernosa. He answers in the negative the question whether we possess any guide at present, in the pathological appearance of the nasal mucous membrane, pointing to the origin of the neurotic symptoms. Why this condition should give rise to ocular symptoms in some persons and not in others he says it is difficult to explain. It may be due to a specially irritable condition of the terminal nerves of the mucous membrane; it may be due to a special neurotic tendency on the part of the individual. We have a practical guide in the possible efficiency of the nasal treatment for the relief of the ocular symptoms, in the use of cocaine. If in these mild cases instillations of cocaine into the nose relieve the eye symptoms, it may be assumed that a simple acute anticatarrhal treatment will suffice for the cure of the affection.

Gruening then gives the details of three cases in which extensive pathological alterations of the nose gave rise to the ocular symptoms, *i.e.*, "hypertrophic swelling of the corpora cavernosa of the turbinated bone found at the anterior and posterior portions of the lower turbinated bones, on the lower edge of the middle turbinated bone. In other cases we have to deal with a stenosis of the nasal passages due to a variety of causes, a combination of hypertrophy of the erectile tissue, cartilaginous excrescences from, and deviations of, the septum." In these cases the cautery, the snare, the knife, and the punch were

resorted to with the effect of usually relieving the ocular symptoms.

The writer was very much impressed by hearing this paper, and was led to pay more attention to the possibility of a relation between affections of the nose and those of the eye, and in the main his clinical observations corroborate what Dr. Gruening has written; in fact, his paper has covered the ground so well that there is not much to be added. It seems to the writer, however, that Gruening lays more stress upon the other symptoms enumerated than he does upon the asthenopia, which is often more aggravated than the other symptoms; by this we mean persistent pain when the eyes are used. Although it may be true, as he has pointed out, as an important symptom in the differential diagnosis, that the burning, smarting sensation of the eyelids is more pronounced in the morning than through the day, it is equally true in my experience that such eyes soon become tired when used, and that painful vision ensues.

During the past five years, especially in my hospital practice, I have paid special attention to this class of cases. After the exclusion of the other possible causes of asthenopia, careful examinations of the nose and throat have been made by my colleagues, Drs. Myles and Burch. Dr. Myles has been kind enough to note down the abnormal conditions which he has found to be causal factors of eye troubles. They are: 1. "Whenever that part of the nasal septum which is situated beneath the bridge of the nose, and anterior to the naso-lachrymal duct, is sufficiently deviated to decidedly compress the tissues against the nasal bones and the nasal processes of the superior maxilla, I have found a certain amount of impaired vision, accompanied with one or more of the following symptoms—blurred sight, blepharospasm, photophobia, fatigue, congestion and thickening of the lids, tension, and pain. These symptoms are produced by the pressure on the longer branch of the nasal nerve, which is only a continuation of the ophthalmic. 2. Hypertrophy of the

middle turbinated bodies, and pressure from the same upon the septum, will occasionally induce many of the symptoms of asthenopia, but they are irregular, and, I think, on account of the pendulous quality of the middle turbinated body. 3. In extremely rare cases enlargement of the inferior turbinated body brings about a condition of weaksightedness. 4. Tumors or abnormal outgrowths exerting pressure on the anterior superior walls of the nose will cause asthenopic symptoms varying with the congestion of the Schneiderian membrane."

A glance of comparison will suffice to show how almost exactly these observations are in line with those made by Dr. Gruening, and therefore how much credit belongs to him for his observations. In the light of these researches, and the by no means scarcity of recent literature and reports of cases to be found, there seems to be ample evidence that there are cases of asthenopia which depend upon affections of the nose, the pathological conditions of which are now pretty well understood.

Among the most remarkable cases of this kind is one published by Dr. A. B. Kibbie in the New York *Medical Record*, 1892, page 462. It reads as follows:

On April 20th, 1890, I was consulted by a gambler, æt. thirty-three, who stated that for nearly a year he had been unable to read or use his eyes for near work without a sense of weariness. During the day they seemed to be "tired," as he expressed it. Examination showed emmetropia and an insufficiency of the externi of 3°. The nasal passages showed a deviation of the septum to the right, and enormous hypertrophy of the left middle turbinated bone, completely filling up the concavity in the deviated septum. The overlying mucous membrane was intensely red, and a ten-per-cent solution of cocaine failed to diminish either the injection or the size of the tumor. After four days' use of atropine, H. 50 of a dioptre was found, but a glass of this number when ordered did no good, neither did 1½° prisms, base outward. The next day, after the removal of the hypertrophied turbinated, he could read, and his headache had nearly ceased; and at the expiration of a week his headaches had entirely

disappeared and he could read indefinitely without pain or annoyance of any kind. The insufficiency, too, had disappeared.

The well-equipped ophthalmologist must be aware of the possibility of the existence of such cases, and their being responsible, in those which show no abnormality of the eye itself, for the asthenopia; and he must either be competent himself to examine for the existence of these pathological conditions in the nose, or, in justice to his patients, must refer them to a rhinologist.

In dismissing this subject it may be of interest to call attention to a table of differential diagnosis between headaches which are due to nasal stenosis and those which are due to eye strain. It is to be found in a very interesting paper by Dr. Edward Pynchon, of Chicago, published in *The Annals of Ophthalmology and Otology* for April, 1893, and which want of time does not allow me to introduce here. It brings out, however, one point not yet referred to in what has already been said, and that is that the eye pain and the headache are quite often unilateral in nasal stenosis.

In this place we may just mention the possibility of the occurrence of some reflex symptoms in the eye from other causes, such as irritation of the fifth nerve from caries of the teeth, and especially from the genital organs. Dr. H. McI. Morton, in an article on "The Influence of the Sexual Organs on Reflex Ocular Neuroses," published in the *Northwestern Lancet*, St. Paul, 1893, xiii., page 328, relates two cases of severe asthenopia, one caused by paraphimosis, the other due to phimosis. It is said by the same writer that masturbation in both sexes affects the eye also by reflex neurosis, as does also uterine disease; but we shall find that the last two mentioned causes can be more properly included in a long list of other exciting causes, and more logically explained than by the rather vague term of "reflex." The writer well recalls a case, sent to him some years ago by Dr. Lewis A. Sayre, of a child who was an almost helpless paretic, had

lost all possibility of locomotion, and was carried about in an invalid-chair. In this case the eyes were affected with photophobia, blepharospasm, dilatation of the pupils, and supraorbital pain, yet no abnormality could be found. There was a tight phimosis, and the operation of circumcision entirely cured all these symptoms, including those of the eye.

The conclusions from these observations which seem to be warranted are: 1. That foci of irritation in the genitals may, through the medium of the reflex nervous system, induce various neuroses of the eye. 2. That while the precise laws which govern the passages of these impulses from other than natural channels are unknown, it is probable that the impulse, greatly intensified by the condition of the reflex centres which are connected by commissural bands, flows on fibres which present a field of less resistance than the usual efferent pathway, and thereby flows into an innocent organ with dire results. 3. That the full appreciation of this possibility should be constantly in the mind of the ophthalmologist, and stimulate him to search for all possible peripheric foci of irritation. 4. That it frequently happens that the trouble disappears on the removal of the remote points of irritation. 5. That illustrations of the fact that points remote from the eyes themselves may be in this way sources of peripheral irritation, which transmit by reflex action to these organs the symptoms of asthenopia, might be multiplied, there can be little doubt, and that the possibilities in this direction are many. We have only sought by enumeration of those which are most commonly recognized to point out the fact.

I now enter upon the consideration of a variety of causes which may give rise to asthenopia, and it might almost be said that their name is legion. The enumeration of a few of those which seem to me the most frequent will, however, suffice; and since all of these causes act very much in the same manner, I may venture, with

some modifications, to classify them all under the general term "neurasthenic asthenopia."

One of the commonest of all causes of asthenopia, in the writer's experience, is uterine disease. This he has already made the subject of a paper which was read before the Section on Ophthalmology of the Academy on January 18th, 1886, and published in the New York *Medical Record* of February 13th, 1886. The influence of uterine disease on the production of visual disturbances, both functional and organic, is now well recognized and universally acknowledged. In the paper referred to I recorded the result of my own observation, and gave the literature of the subject so far as my research at the time went. It is not my intention to attempt to even refer to all that has been written on the subject since then, but there appeared, very shortly after my own paper, one by Dr. Clifton S. Morse, of Newark, Ohio, entitled "Asthenopia coincident with and largely dependent upon Uterine Affections." This paper, to which I shall have to refer again, was published in the New York *Medical Journal* of January 22d, 1887. My paper was of a practical and clinical nature, and gave briefly the results of my own personal observations in a number of cases which were taken from my records, and also the observations of several of my colleagues engaged in the practice of gynæcology who had been kind enough to favor me with their views. This paper, while pointing out that the eye diseases thus engendered, from being at first only functional, might in time lead to organic disease, dealt more especially with asthenopia from uterine affections. So far as it was possible the nature of the uterine disease was given —imperfectly when the account of the case was given by the patient, and in other instances the diagnosis was more accurate and was obtained from competent gynæcologists. I addressed a note to several specialists, two of whom replied. The following letter sent me by the late Dr. J. B. Hunter furnishes the strongest and most conclusive evi-

dence in favor of the existence of uterine asthenopia. He writes:

"It would take some time to hunt up and sort out the cases, so I will simply state the fact that I have in many cases been so accustomed to hear the eyes complained of where there was chronic uterine disease, that I believe there is no doubt as to the connection between the two ailments. In bad cases of lacerated cervix uteri, where there is much dense cicatricial tissue, patients commonly complain of weakness of the eye; that they tire easily in reading and writing, and often ache. These symptoms I have often known to disappear within a few months after an operation for the cure of the lacerated or indurated cervix. The reason why I have considered the disease of the eye sympathetic is that such disease has again and again passed away without direct treatment on curing the uterine disease. Pain in the top of the head is one of the commonest symptoms in uterine disease, and vertical pain is often accompanied by either pain or weakness in the eye."

Since I wrote this paper my observation and experience have only served to confirm my opinion as to the frequency and importance of the relation between all uterine affections, both functional and organic, and those of the eye, especially those which are commonly designated as asthenopia. It does not come within the scope of this present writing to show that graver structural changes in the eye are often due to the same cause, but to this I have referred in my former paper, and Mooren[1] and others have also written about it.

I shall now proceed to give an illustrative case or two, selected from the twenty published in my paper, and those selected come within the proper scope of this paper, in that they had neither ametropia nor insufficiency of the eye muscles to account for their asthenopia.

CASE III.—Married, age not given. For a long time has suffered from asthenopia and severe supraorbital

[1] "Disturbances of Vision and Uterine Diseases," Archives of Ophthalmology, vol. xi., No. 3, p. 281.

neuralgia; of late more severe in the right eye, the lid of which was swollen. Examination shows very slight ptosis of the right upper lid, some lachrymation; pupils respond well to light; ophthalmoscope shows no abnormality except that the right disc is paler than the left; E. right $\frac{20}{40}$, left $\frac{20}{30}$; glasses afford no relief. She has suffered for a number of years from uterine disease, the nature of which she does not know, and also from profuse leucorrhœa.

CASE V.—The wife of a physician, married, aged twenty-nine; suffers from most aggravated form of asthenopia after the slightest use of her eyes, so that for years she has not been able to read for half an hour at a time. The refraction is emmetropic; accommodation normal; V. $\frac{20}{20}$; has anteflexion and dysmenorrhœa, and is sterile. She has been under treatment for a long time by the use of pessaries and topical applications. Her eyes are always worse during menstruation. This lady has been under my observation for years, but she has never been relieved from eye fatigue except occasionally when there is an amelioration of her uterine trouble.

CASE II.—Married; thirty-four years of age; one child fourteen years old. She complains of asthenopia which gives her very much annoyance upon the least attempt to use her eyes; emmetropia; vision is $\frac{20}{20}$, but feeble accommodation; gave her + 1 D. to read with, which to some extent gave relief in the use of her eyes. Uterine disease since birth of her child; also dysmenorrhœa and occasional hæmorrhages. The case was diagnosed by her physician as laceration of the cervix. Operation made the eyes better, but recurrence of the uterine symptoms made the eyes worse again. She now consulted Dr. Thomas, who found fungosities of the uterus, which he removed by the curette. Since then she has had neither uterine disturbance nor asthenopia; menstruation normal and painless; moreover, since the cure of the uterine disease she has laid aside her glasses.

There are still more emphatic examples of the truth I have asserted here in the paper, but these may suffice; those wanting further evidence will find it in my published paper.

In Dr. Morse's paper, to which I have alluded, he refers to mine, and says that while I enter into the subject at some length and recognize asthenopia as dependent

upon, and more or less resulting from, uterine disease, I do not place any degree of stress upon the occurrence of what he calls "menstrual asthenopia." This, he says, will be found, with or without ametropia, to be extremely common, annoying, and quite intractable to ordinary treatment. Since it is true that in my paper, my attention not having been especially called to this phase of these cases, I only incidentally alluded to asthenopia being somewhat aggravated at the occurrence of menstruation, I shall take pains to give its peculiar characteristics, as depicted in Cases III., IV., and V. of his paper:

CASE III.—Widow, age thirty-four. This patient is of the well-nourished, round, and rosy sort; no insufficiency; fundus normal; the correction of 1 D. H. brought vision to $\frac{20}{20}$. However, for a few days before and after, and during the menstrual flow, her eyes are in a truly pitiable condition. At these times her asthenopia is of the most aggravated form, all visual effort being attended by intense supraorbital pain and photophobia, followed by conjunctival hyperæmia. When at its maximum she is obliged to remain in a darkened room during the daylight and wait until it subsides. However, shortly after the flow has ceased her eyes become better and she has a period of comfort and rest. I directed her to the office of one of my gynæcological friends, with a rather surprising result—no uterine trouble could possibly be found; there was no discharge, nothing, in fact, but the most tantalizing health. No treatment being possible, the case remains as found.

CASE IV.—Single, age eighteen; has been troubled with supraorbital neuralgia, photophobia, and persistent headache at each menstrual epoch since her fifteenth year. She consulted me for the purpose of obtaining an order for glasses to correct a presumed error of refraction. Vision was found to be $\frac{20}{20}$, fundus normal, and no insufficiency. I examined her eyes several times during the menstrual flow; the retinal veins were very full, but otherwise everything was as before. There was some increased intraocular tension, however, which was relieved somewhat by the alternate use of eserin and pilocarpin. No lasting effects were produced by any treatment. There is no history of uterine disease. Two sisters of the patient are similarly affected.

CASE V.—Single, age twenty-five; has been troubled with a formidable array of asthenopic symptoms for years, and these have been much intensified at the menstrual epoch. With minus 50 dioptre cylinder, axis 180°, vision was $\frac{20}{20}$. A physician has been treating her for some time for a catarrhal discharge, perhaps not specific, which he says he has entirely cured. Her asthenopia is very little lessened in severity, although its character is materially changed. While formerly she suffered more or less all the time, now there is very little inconvenience, except at the occurrence of the monthly flow. The ophthalmoscope shows nothing abnormal; neither is there insufficiency. Here I also found increased intraocular tension, and the near point moved further away. Neither eserin nor pilocarpin produced any lasting effect.

I have given these cases because they seem to me to be of very great interest and importance, and tend to show that asthenopia may be one of the protean symptoms which accompany the periodical menstrual molimen, even when this act is not retarded or otherwise interfered with by the coexistence of uterine disease and consequent dysmenorrhœa. So far as I know to the contrary, Dr. Morse may be credited with having first accurately called attention to the existence of this form of asthenopia.

To sum up these observations, then, it may be looked upon as an established fact that in certain cases there is a direct relation between irregularities in functions and diseases of the uterus, and concomitant affections of the eye. The eye affection may be merely functional—which is commoner asthenopia—or there may be organic disease. This condition exists when there is no ametropia, although in many there is a paresis of accommodation. These conditions may be properly included under the name of uterine asthenopia and "menstrual asthenopia" (Morse). Whether these ocular symptoms are properly to be explained, as I have done in my paper, as only due to reflex effects of the uterine disturbance on the organs of vision, is perhaps doubtful, and for this reason I would rather seek to classify them in the category of neurasthenic, and see whether, especially in chronic uterine dis-

ease, it is not a want of proper nutrition of the entire nervous system, which gives rise to the condition now known as nervous prostration, which is responsible for the eye symptoms as well as for many others.

The best division into groups of the causes of asthenopia, the writer thinks, is that made by Noyes in his recent text book. It embraces (1) refractive errors, (2) muscular defects, (3) neurasthenia, and (4) reflexes from the nasal mucous membrane. Noyes embraces in the term neurasthenia a number of exciting causes—viz., the conditions which give rise to the affection of the nervous system which is known under this name, and for which the eye symptoms are only a part of this very complicated neurosis. We believe, moreover, that we may include in this category all cases of asthenopia, even although many of them do not show all the symptoms of that form of asthenopia described under the name of neurasthenic, and formerly known as either hyperæsthesia or anæsthesia of the retina. They are, in reality, only different expressions of form and degree of the general nervous diathesis. Thus, *asthenopia nervosi* and hysterical asthenopia of some authors, and the *copiopia hysterica* of Förster, are really but one of the forms of neurasthenic asthenopia; in other words, it seems confusing to describe both a hysterical asthenopia and a nervous asthenopia, which are frequently associated, and in both of which the psychical factor plays a great part. I will only here point out that very recently cases have been described by Berlin,[1] under the name of "dyslexia," which might easily be confounded with asthenopia. In these cases reading often becomes impossible, even after a few words have been read, without there being any blurring of the print or any pain. In several of these cases autopsy has shown the presence of disease in the left cerebral hemisphere, more particularly in the neighborhood of the third frontal convolution. In some of the cases, too, which we may include in the neurasthenic

[1] Fuchs, "Text Book of Ophthalmology," 1892.

group, the eye fatigue may depend upon impairment of the accommodation, which, too, is only a result of some acute disease, as typhoid fever, diphtheria, scarlatina, and the like, and in which we may too often have a train of other nervous symptoms as well. There is still another point which should not be lost sight of, but nevertheless is not given the importance it deserves—that is, we may have in these affections defects in the eye muscles themselves, which will disappear as the patient recovers. Here some of the good results of the much-lauded partial tenotomies come in. The President of this Academy has often, in conversation with the writer, referred to the importance of this. We can include, therefore, a large number of exciting causes in the neurasthenic list, if we only extend its definition to one of degree, such as uterine affection and its consequences, chlorosis, menstrual irregularities, hysteria, masturbation and excessive sexual indulgence, exhausting pollutions, chlorosis from other than uterine causes, severe acute diseases, such as typhoid fever, diphtheria, scarlatina, syphilis, and hereditary nervous disease. This list is suggestive; any of the causes herein embraced only suggest others of a similar nature and of great variety.

Under the title of neurasthenic asthenopia and so-called *anæsthesia retinæ*, Dr. Herman Wilbrandt,[1] of Hamburg, has written a most important paper, which is by far the most comprehensive and valuable article bearing on this subject which has yet been published. He says that both the terms, anæsthesia and hyperæsthesia of the retina, are objectionable, the choice of the name being determined by the relative prominence in each particular case of increased irritability on the one hand, or decreased functional ability on the other, and he therefore suggests the use of the term "neurasthenic asthenopia." Now, the pathological bases for these symptoms are one and the same, and are simply a local manifestation of that nervous diathesis which was first described by our distinguished

[1] Archives of Ophthalmology, xiii., p. 428.

countryman, the late Dr. George M. Beard, and is essentially an impoverishment of the nerve force, dependent upon an excessive waste of nerve tissue. The best evidence of the merit of his work is that it is referred to by Wilbrandt; for when a publication from the pen of an American is quoted by a German authority, there must be something of unusual merit in it.

The subjective eye symptoms of this condition have been arranged by Wilbrandt as follows: Peculiar contraction of the visual field, rapid disappearance from view of the object which is being fixed, diminution of central vision, sudden attacks of obscuration of vision and processions of scotomata, visual hallucinations, lack of fixation of the optical memory images, inability to read for any length of time, weariness on the part of the muscles of the eye and eyelids, and positive insufficiency of the interni. In addition to these there may be defective accommodation, intolerance of light, and impairment of vision in the dusk or through tinted glasses. The most characteristic symptom is a peculiar contraction of the field of vision, which at the beginning of the examination may present normal limits, but, as the examination proceeds, contracts most markedly, above and below the horizontal meridians, because the nervous apparatus becomes fatigued. If the patient is allowed to rest, and the examination is repeated, but the test object is now moved along a meridian in an opposite direction to that in which it was moved in the first examination, a second field will be obtained which is called by Wilbrandt "the counter field." The most contracted part of the counter field lies on the same side as the most expanded part of the previous visual field. The visual field for colors exhibits about the same conditions as those for white. Patients affected with this condition are for the most part women, and are often the subject of ovarian and uterine disease, hysteria, chlorosis, and so on. Fair types of the affection are also seen in men.

In addition to the local symptoms described in connec-

tion with the visual organs, these patients have a host of symptoms, generally indicative of their defective nutrition and nervous tone. These symptoms are far too numerous for us to enumerate here, but who does not know them? Let such read Beard or the paper referred to here. Simple mention of them would be to take up more time than we can command; in other words, they suffer from a series of symptoms classified under the term neurasthenia—increased excitability of the nervous system with a tendency to rapid fatigue.

The treatment of such cases is directed largely to the general condition, and includes the so-called "rest cure," viz., rest with seclusion, forced feeding, massage and electricity, tonics of various kinds, with ascending doses of nux vomica and strychnia provided the retinæ are not congested, and gradual exercise. Although tinted glasses are to be sometimes recommended, they are not always advisable lest the affected eyes become too much accustomed to the modified light afforded by them. The gradual exercise of the eyes by the Dyer-Derby method is to be recommended, as is also the use of weak convex glasses, and if errors of refraction or muscular insufficiencies exist they should be corrected. But spectacles of all types, and tenotomies partial or complete, do not suffice to cure these cases, which are constantly subject to a relapse of their symptoms. In one case of this kind, however, sent me by Dr. Coe, in which all the symptoms above enumerated were present, tenotomy of the interni, whether by "suggestion" or not, did do a great deal of good. It is not claimed that in all cases of asthenopia from a nervous cause we have all of these symptoms present, although many of them may be found when looked for carefully; but there are different degrees of asthenopia which can be best accounted for by varying amounts of perversion of the nervous system—varying from simple paresis of the accommodation, as seen after diphtheria and other acute diseases, to those of extreme neurasthenia which embraces all the symptoms herein described as pe-

culiar to the eyes, as well as the other symptoms characteristic of this affection. We venture, therefore, to suggest, provisionally at least, that this is a rational and convenient classification. If time allowed we might go more into detail regarding the peculiarities of some of these kinds of asthenopia, such as occur in masturbation and sexual excesses, for example; but they are, after all, essentially those included in the above category.

Seguin [1] suggests that the majority of cases of cerebral hyperæmia which present symptoms of eye strain are due to the persistent use of weak neuro-muscular organs, and that errors of refraction, which are so important a factor in migraine and headaches, are here of little importance.

We may briefly summarize the conclusions reached in this paper as follows:

1. Asthenopia may exist quite independently of errors of refraction or affections of the eye muscles.
2. Asthenopia may be due to toxic causes.
3. It may be due to excessive use of the eye.
4. It may be from various reflex causes.
5. A large number of cases are due to irritability and exhaustion of the nervous system, embraced under the term neurasthenia.
6. That the rational treatment of all such cases consists in arriving at a knowledge of the causes, of which asthenopia may be considered but a symptom, and the correction of the same, together with rest, systematic exercise of the eyes, and the use of feeble convex glasses where there is paresis of accommodation.

It is the tendency of modern ophthalmology to seek a local cause for all disturbances of vision, and to use local treatment exclusively; but this has not met with the unqualified approbation of those who have sought to be physicians as well as ophthalmologists. In these days of the rapid growth of specialists, when oculists, like nations, are born in a night, begin their careers without any

[1] New York Medical Journal, lvi., p. 617.

adequate knowledge of general medicine and surgery, and, after a few weeks or months of study in a post-graduate school or an eye infirmary, come out, forsooth, as fully fledged specialists, this is hardly to be wondered at. Because the eye is shaped like a sphere or globe and moves in its own orbit, are we to consider it as an independent organ, acting of itself without any relation to the rest of the body, or rather as only a "part of one stupendous whole"?

In concluding this paper we make a plea to our colleagues for a wider and more thorough knowledge of all the laws which govern general physiology, pathology, and therapeutics, and a wise interpretation of their relation to the special diseases we are called upon to treat.

DISCUSSION.

DR. E. GRUENING.—I am heartily in accord with everything that Dr. Pooley has said regarding the importance of certain ocular symptoms, and their dependence upon or relationship to other organs of the body. Of late we have been accustomed to hear these symptoms discussed in too narrow a spirit; the eyes alone were considered, and the fact was lost sight of that these organs help to make up the complex human system.

Accommodative asthenopia, which was one of the forms of the affection referred to by Dr. Pooley, is often overlooked. A patient who is recovering from a debilitating illness may be so weak that she is hardly able to stand on her feet, so she remains in bed and reads a novel, perhaps, all day and part of the night. Next to the muscles of the heart, the ciliary muscle is perhaps more constantly in action than any other. It therefore follows that in a case like the above it becomes exhausted and may give rise to symptoms which I will not take time to mention here. This exhaustion of the ciliary muscle may also lie at the bottom of menstrual asthenopia. Women who suffer from uterine disease are apt to have an exacerbation of the symptoms during the menstrual epoch, during which they spend their time in bed reading novels. Partial tenotomies and the correction of minimal degrees of astigmatism do not benefit patients suffering from accommodative asthenopia.

The paper which Dr. Pooley has read is of importance, because it impresses upon us the fact that we are still a part of the general profession.

DR. C. S. BULL.—All I have to say may be said in a very few words. First of all, I want to thank Dr. Pooley for the privilege of listening to his paper, which I regard as a very timely one. My interest in the subject is enhanced by the fact that I am connected with one of the large special hospitals in this city and with one of the schools of instruction, and here I am continually asked by students who are not expected to know any better, and by general practitioners who are expected to know better, questions in regard to points which were discussed in detail by Dr. Pooley. I also wish to thank Dr. Pooley for his clear and emphatic statement—I do not know that I can make it more so—that a specialist, whether he be an oculist or a specialist in any other branch of medicine, is only of value as he is a good general practitioner and has a sound knowledge of general medicine.

Just a few words in regard to several points suggested by Dr. Pooley's paper. I shall not dwell on the subject of nasal disease as a cause of asthenopia, as that has become a well-recognized fact. I have noticed in my practice that asthenopia is sometimes the initial symptom marking the onset of what we call simple chronic glaucoma; in such a case there may be no error of refraction, no muscular deficiencies, no intraocular evidence of the disease, no increased arterial tension, no narrowing of the field of vision; there is simply pronounced asthenopia, and I have been forced to recognize in some cases that asthenopia is the first symptom—or rather complex set of symptoms—in simple chronic glaucoma. I have not come across this fact in my reading.

Another point to which I desire to refer is that long-continued use of the eyes under the electric light—either the arc or the Edison incandescent light—gives rise to asthenopia. There are many clerks in this city who work all day under these lights, and not a single week passes over my head without my seeing at least one or two patients suffering from aggravated asthenopia due to this cause, and in which there are no intra- or extraocular conditions to account for the production of their symptoms. My conclusion that the electric light is at the bottom of the trouble in these cases is proved to be cor-

rect by the fact that all the symptoms disappear when the patients discontinue their work.

Asthenopia, without any error of refraction or muscular anomalies, may be one of the earliest symptoms of diabetes, even before any traces of sugar can be discovered in the urine by a most careful examination. I have in a number of instances made an early diagnosis of diabetes in this way. I can recall six or eight such cases within the past year.

One more remark regarding that protean group of symptoms to which we have applied the name of neurasthenia. Many such patients, particularly females, complain of vague ocular symptoms; a careful examination of the eye shows nothing abnormal, and we are forced to accept the theory that the eye symptoms are probably due to some morbid condition of the general nervous system, to a faulty general circulation, etc. Sometimes this disturbance of the circulation can be seen in the intraocular vessels by means of the ophthalmoscope. I have seen cases in which the calibre of the vessels, especially that of the veins, increased and diminished during the ophthalmoscopic examination. The symptoms of the asthenopia were most marked when the veins were the fullest. I do not know why this was so.

The conclusion to which I have come is that there is a certain proportion of cases that come under our care as ophthalmologists in which a most rigid examination with all our modern instruments of precision fails to reveal any organic or functional difficulty, and yet these patients present a group of symptoms which I formerly considered were due to errors of refraction in every case; I am now well aware that this is not so, and that in order to cure their eye symptoms their general system must be carefully looked after.

DR. R. H. SAYRE.—Although I know very little about asthenopia, I can, however, corroborate Dr. Pooley's remarks as to its being due at times simply to genital irritation, having seen a certain number of such cases. Dr. Pooley, in his paper, mentioned one case, referred to him by my father, where relief of phimosis cured the asthenopia. I might recite another: A number of years ago, shortly after my father read his first paper on the production of various paretic phenomena as the result of reflex genital irritation, a patient was sent to him by Dr. Knapp. This gentleman was unable to use his eyes for any length

of time, and could read only the largest type. He had consulted various oculists, both in this country and abroad, without relief.

Not finding any organic ocular lesion to account for his symptoms, but ascertaining that he suffered from a very tight phimosis, Dr. Knapp referred the case to my father, thinking the ocular symptoms were but a reflex condition due to the disturbed genital system.

He was circumcised, and at once the asthenopia disappeared. He was able to read the finest type with ease, and has remained well ever since.

I think we all coincide with Dr. Pooley's remarks about specialism. Many neurologists are of the opinion that genital irritation does not produce pronounced nervous symptoms; I have, however, seen such sources of irritation give rise to profound reflex nervous symptoms in various organs of the body. I feel more convinced, the older I grow, that that man is best equipped to practise a special branch of medicine who has the most thorough knowledge of the entire human economy. His general knowledge of medicine must be a broad one. Otherwise he is apt to dig the hole in which he is searching for truth so deep that he cannot see over the top of it to learn what is going on in the outside world.

DR. R. C. M. PAGE.—I have listened to the paper of the evening with great interest, not only for its intrinsic value, but also because of its importance in showing how careful the physician should be not to mistake the symptoms of a general disease for a merely local trouble. I remember once hearing the late Dr. Agnew remark, during the consultation regarding a case, that the longer he lived the more he became convinced of the importance of looking into the general condition of the patient, rather than to dwell too much if not solely upon the treatment of local conditions. This fact was very forcibly impressed on my mind by the following case: A gentleman applied to an oculist in one of our neighboring cities for the relief of certain eye symptoms. Failing to get this relief, he came on to New York and requested me to give him the name of a competent oculist. I was led to examine his urine, and found it loaded with sugar. Diabetes, therefore, was the cause of his eye trouble. We know that asthenopia and general muscular paresis are early signs of diabetes.

I think, Mr. President, that the speaker has been par-

ticularly happy in the selection of a subject which shows how necessary it is not to mistake the symptoms of some general disease for a local trouble.

DR. J. R. GOFFE.—I suppose Dr. Pooley desires to have me discuss his paper from the standpoint of the gynæcologist. I only regret that some one who has had a wider experience than myself is not here to represent that department. I think we are all in sympathy with the broad views entertained by the reader of the paper, and I am fully in accord with his statements regarding the relation between uterine trouble and eye symptoms. I have usually regarded this condition of asthenopia in cases that have been under my care for uterine trouble as being due to a weakened state of the general nervous system. I have had one case of this kind under my observation during the past four years which puzzled me a great deal, as it also did the oculists. The patient is a young woman, aged thirty years, who originally called upon an eye specialist, complaining of painful vision. He examined her eyes, and on account of the appearance of the retina he became so much alarmed—fearing that she had a brain tumor—that he would not allow her to leave his office, but sent for her mother to escort her home. He then referred her to a neurologist, and after a long and careful examination they both agreed that the patient had a tumor of the brain and that it was a very serious case. She visited both of these gentlemen every ten days for six months, who put her on large doses of potassium iodide, etc. It so happened that she was acquainted with one of my patients, who suggested that she might have some uterine trouble and sent her to me. I discovered that she had a retroverted uterus which was bound down by adhesions. I stopped the large doses of the iodide (she was taking eighty grains a day) and gave her local treatment, a rigid regimen of exercise, general tonics, and appropriate nourishment. She immediately began to improve. About this time her sister consulted me; she was a widow who was addicted to masturbation, a practice in which I suspected the original patient also indulged. While talking to her about her sister I took pains to dwell upon the evils of the habit, which I think so much impressed her that she abandoned the practice. I believe that her trouble was due principally to masturbation. In the course of one year she was quite well and her eye symptoms entirely disappeared. The neurolo-

gist and the eye specialist, both of whom I saw, were very much surprised to learn that a woman could be cured of a tumor of the brain by treatment applied to the uterus. In that case the connection between the uterine trouble and the eye symptoms was especially marked.

DR. R. C. MYLES.—I want to thank Dr. Pooley for his paper. It is from this class of papers that we derive the greatest benefit. Rhinological asthenopia is much more definite than any other form. It is the only form of the disease that can be produced at will, and hardly admits of any discussion. Asthenopia is also in many instances due to bad nutrition or to general debility.

I often meet with asthenopia in patients who consult me for nasal trouble, and on referring them to an oculist he sends them back, after a thorough examination, with the statement that nothing abnormal has been discovered in the eyes. Occasionally I find the cause of the eye symptoms in the nose or the accessory sinuses. This suggests the fact that patients are too frequently operated on for supposed nasal trouble when such operations are not called for. It is very common for patients to have nasal defects without any asthenopia. Permanent or definite pressure upon the bones in the anterior upper part of the nose or upon the nerves by tumors or hypertrophic tissue will produce asthenopia, and in such cases an operation is called for.

In many cases of asthenopia due to nasal trouble, the painful eye symptoms referred to by Dr. Pooley are absent. The patients complain of blurred or disturbed vision. The oculist fails to discover anything to account for this, and the seat of the trouble seems to lie in the nasal mucosa, the accessory sinuses, or in the turbinated bodies. In one such case of blurred vision coming under my observation the patient was seen by two prominent ophthalmologists, who failed to find anything to account for it. His trouble was especially marked in the morning. On examination I found that the patient had a latent empyema of the antrums of Highmore. I perforated each and found a diseased area nearly two inches in diameter. On the left side the part under the orbit was covered with fungating granulations, thick and spongy in character. Since the operation his eye symptoms have entirely disappeared.

THE PRESIDENT.—I simply wish to say that, in common with you, I have enjoyed the spirit and comprehen-

siveness of this paper, and with its general drift, so far as my humble opinion goes, I am entirely in unison. Anybody who has done me the honor to read what I have written on this subject of asthenopia—and I sometimes fear I have written too much—knows my opinion of that specialism that is ignorant of general medicine. Ignorance of what asthenopia really means has led to practice most humiliating in this special branch of medicine.

One of the earliest symptoms of locomotor ataxia is an inability to use the eyes, and I have in a number of instances diagnosticated that disease from the ophthalmic symptoms alone. Yet there are some who would undertake to remedy such ocular defects by an operation. Still, the specialist is not alone to blame. Sometimes the general practitioner is so ignorant of the eye symptoms found in certain diseases, such as diabetes, that he sends the patients to the oculist for treatment. If we would only bear in mind what Donders and others meant by asthenopia, we would not attempt to convert the ordinary eye into the ideal one. I am a disciple of Helmholtz and other great scientists who have measured eyes most exactly, and I do not think there is such a thing as an ideal emmetropic eye. I do not think such a thing exists, excepting on the blackboard or in the text books, for the purposes of study. I do not think that a muscular anomaly has anything whatever to do with producing asthenopia, except in a secondary way. I pay no attention to the diagnosis of muscular insufficiencies. I shall not, however, exhaust your patience by going into this more thoroughly, as my views on the subject are fully set forth in numerous publications which I expect to elaborate in the near future. I also claim for myself, in contradistinction to my German friend in Hamburg, the nomenclature of neurotic asthenopia, about which I have written very fully. Neurasthenic and neurotic asthenopia, I suppose, are comprehended under the same heading. The truth contained in Dr. Pooley's paper will gradually infect the whole band of oculists in this country, and we shall cease to hear so much about curing asthenopia and general diseases with prisms, partial tenotomies, and so forth.

DR. POOLEY (closing the discussion).—I desire to express my warm thanks to the gentlemen who have taken part in this discussion.

I am entirely in accord with what Dr. Gruening said regarding the accommodation being at fault in many cases

of asthenopia in women suffering from uterine trouble or general weakness, and it seems to me that the explanation he gives is a most ingenious one. Such patients are usually sluggish and indolent in their habits, and enfeebled accommodation may result. An examination of the patient's power of accommodation is often neglected by the younger men.

Dr. Bull brought out a very interesting statement in regard to asthenopia being the initial symptom of glaucoma. I have noticed this in a number of asthenopic eyes where I found no error of refraction. In diabetes and locomotor ataxia we have asthenopia, and one of the earliest symptoms is the weakening of the eye muscles.

I was extremely interested in the case narrated by Dr. Goffe, and have been wondering what condition of the eye was revealed by the ophthalmoscope which should have led the neurologist and ophthalmologist to believe that they had to deal with a case of brain tumor. Probably it was hyperæmia of the optic disc, which we know may exist without any cerebral lesion.

The points referred to by Dr. Myles are very important. In my experience grave diseases of the accessory sinuses, an example of which he gave, are not so usually accompanied by asthenopia. I saw the case referred to, and examined him with painstaking care, and failed to discover any error of refraction.

WESLEY M. CARPENTER LECTURE.[1]

THE SURGERY OF THE HAND.

By ROBERT ABBE, M.D.

My interest in the surgery of the hand was first stimulated in 1875, when, in assisting Dr. Weir in a resection of the wrist, I read Liston's enthusiastic and scientific article upon that subject, in which he spoke of the marvellous mechanical perfection of this particular joint.

No one can contemplate the perfect hand without wonderment and admiration, but, like the use of our eyes and ears, we think little about it unless its use is impaired.

Many of the surgical troubles that occur to the hand—fractures, sprains, etc.—have their counterpart elsewhere in the body, and we shall not consider these this evening. Nevertheless there is great need of wider knowledge by general practitioners of the importance and gravity of such common troubles as poisoned cuts and pin pricks of the thumb and little finger, the suppurative inflammation from which spreads along the tendon sheaths to the common bursa of all the flexor tendons above and below the wrist. This causes permanent loss of usefulness of more hands than are destroyed by all other causes together. A bad result can only be prevented by quick use of the knife and antiseptics.

Of very great interest among hand troubles, of which I find a widespread ignorance among physicians, is one which has attracted my attention specially during the last twelve or fifteen years, and which is illustrated by some of the charts here displayed—namely, Dupuytren's contraction of the palmar fascia.

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[1] Delivered November 20th, 1893.

The general surgical aspect of the malady has received most careful consideration by Mr. William Adams, whose very large experience in this particular disease has rendered him the first authority upon the subject, and it is needless for me to more than refer to the second edition of his valuable work, issued last year, for most of the essential points upon it. There are some points, however, that have come within the scope of my experience on which my judgment differs somewhat from his, and to these I will direct especial attention.

In two previous articles written by me upon this sub-

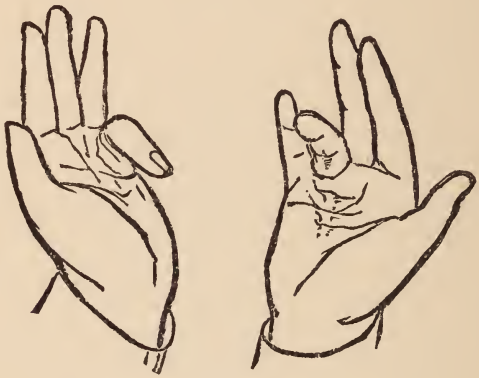

FIG. 1.

ject, first in 1884 (see the *New York Medical Journal*, April 19th and 26th), and later in the *Medical Record*, March 3d, 1888, I proposed a theory of its causation which was entirely at variance with the gouty theory generally accepted and since reiterated by Mr. Adams.

Reasoning from numerous cases of the neuroses and neuralgias which the patients with Dupuytren's contraction were often subject to, I satisfied my own mind, at least, that there was a strong probability that traumatism of the nerve ends in the palm, reflected to the central nervous system and thence to the opposite hand, would perfectly account for the course of events as seen in this class of cases.

Mr. Adams and Dr. Keen, of Philadelphia, have both lingered over the theory of its gouty origin, strongly impressed by certain phases of rheumatic development in the affected hands, and by the occasional hereditary tendency of the disease.

My own experience, based at that time upon about twenty-five private cases and fifteen additional public hospital cases, led me to discard the gouty theory because of the almost universal absence of gouty inheritance of the patients who came under my observation. It was not rare

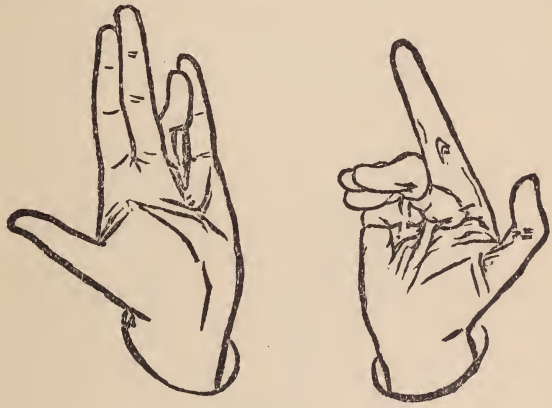

FIG. 2.

to find a history of rheumatism in the families of the patients, but extremely rare to find a case of typical gout.

In both my papers I detailed many cases of striking neuralgias and neuroses incident to the course of the contracted fingers. It is curious to me that Mr. Adams is able to say that in the course of his experience in England he has never met with any such cases of painful neuralgias and neurotic affections extending to the shoulders and spine. He naïvely remarks that while I have "asserted that in English society gout is an almost universal legacy, and in America typical gout comparatively rare, but rheumatism more common," he "cannot help thinking that distressingly painful neuralgias and various neurotic affections are more common in America than in England."

My subsequent experience, based on an additional twenty-five cases in private practice during the last four years, into the histories of which I have been able to search, leads me further away than ever from the presumption of gouty origin. A few striking cases of neuralgias have occurred among these, but none so pronounced as several that are quoted in my first two papers. Several cases in my experience have resulted from distinct traumatism and scars.

I shall briefly recount a few typical cases to illustrate my position, and I shall be greatly surprised if the future experience of others having their attention called to these associated symptoms will not also reveal many that have

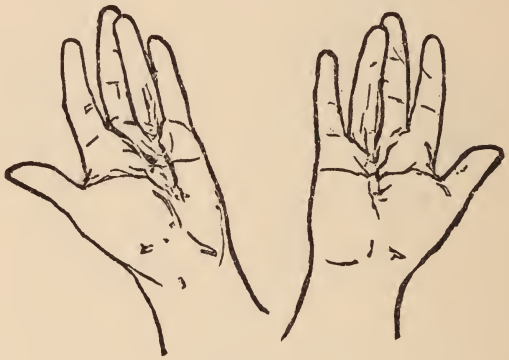

FIG. 3.

been overlooked by them. Except for the typical gouty manifestations in the feet and hands, it is easy to say gout is the cause of many clinical symptoms, and hard to prove it in these contracted finger cases. The trouble is certainly absolutely rebellious to antarthritic remedies and does not come generally in gouty subjects. There is no exact analogy to this palmar contraction in gouty manifestations elsewhere. Tophi are never seen in these palmar contractions, no matter how long they may have existed.

The theory of gout, so far as I can see, is purely an assumption. On the other hand, the disease affects a

member whose tissues are highly endowed with nerve ends. It is associated with pure neuralgias. Its neuralgias are often relieved as if by magic by an operation on the bands. It prefers to attack the ring and little fingers, the most subject to bruises.

"The joints of these hands are generally in a healthy condition," says Mr. Adams. I myself have but once seen a truly gouty knuckle in these patients. The fact that many patients can refer the contracting band to injury and sometimes show scars is an argument in favor of this probable causation in all cases—*i.e.*, traumatism. Such cases as the following are not uncommon:

Ten years ago a patient was precipitated down an elevator, hanging on the chain for two stories. He did not experience any special bruise, except that the skin was slightly cut on the left hand and torn a little on the right. Three or four years later the ring finger in each was drawn down a little in the palm, the left soon touching it.

Several of my patients attributed their contractions to the driving of horses; others to boat-rowing and other injuries. One typical bruise may be illustrated as follows:

Mr. O., aged fifty-three years, a most intelligent observer of his family history, denies rheumatism or gout on either side among his ancestors, and is conversant with his family history for several generations back. Ten years before this trouble began he used the palm of his right hand, on the little-finger side, to knock up the lever of a heavy safe door, and has kept up the practice ever since. In two or three years the contraction began in the little finger of that hand, and it has progressed ever since. Dull aching was noticed in the morning at the seat of trouble. Two years later the knuckles of all his joints of the affected hand began to act as if subject to subacute rheumatism, and two years ago the left-hand knuckles of the corresponding fingers became red, swollen, and showed some disposition to ache. This phase of rheumatism, as pointed out and fully illustrated in my first paper, is a distinct neurosis.[1]

[1] Of other cases attributable to accidents I would mention that of Mr. P. K., who, climbing a ladder, had a spiked piece of frozen mortar clinging to a

An interesting illustration of neurosis was shown by Mr. W., who had a sharp band developed in the middle finger ten years before. Some contraction had also developed in the left palm later. The patient emphasized a curious neurosis that had been present in the right arm most of the time, especially on awakening and for some time afterward—*i.e.*, a sense of great increase in size, as if the shoulder and arm were distended to twice their natural size, giving a strange and unnatural feeling; occasionally he has pain about the left shoulder. In 1885 I operated, dissecting out the band throughout the palm. There were three purplish nodular swellings along this cord. It was done under cocaine and painless. The finger came out perfectly straight. Two years and a half later the hand was absolutely free from contraction; the fingers could be even bent backward a little; the cicatrix was soft and white; he had had no pain for two years. The neurosis of his arm entirely disappeared. Moreover, the left-hand bands, that had not been operated on, became softened a little and gave no trouble.

A second case was of much interest. Dr. W., a distinguished physician of this city, with absolutely no inheritance of rheumatism or gout, attributes his contractions to the use of the rough head of a cane. His father also had had the same trouble. In his right hand, for fourteen years, he had a contracted ring finger, bending down well toward the palm. Recently he had suffered from brachial neuralgia and deltoid and cervical pains, like lumbago,

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round pierce his palm over the ring finger. The callus never went away entirely. Typical Dupuytren's contraction ensued. After two or three years progressive weakness of the ulnar fingers ensued, diminishing his grip and associated with writer's cramp.

In another, a civil engineer had a long series of stakes to put into the ground, and pressed them hard with his palm. Next day he had a sore palm, and dates his contraction from that.

I have seen but one case where Dupuytren's contraction was secondary to neuritis.

A man of thirty-seven years had his forearm caught between two cars, so as to jam the elbow at one end and the palm at the other. Subsequently he had pain of ulnar and progressive partial paresis, with atrophy of the thenar eminence and development of true Dupuytren's contraction of the fascia over the ring and little fingers, which I dissected out with much relief, but ulnar neuralgia with swollen nerve, easily felt at the elbow, continued as long as he was under observation.

on the affected side. His left hand began, seven years after the right, at the same point precisely. There was a burning sensation in the palm, and commencing neuralgia of his arm. The right hand showed bands running to the little and ring fingers; the left, bands, faintly outlined, going to the same fingers.

This illustrates the almost universal fact that both hands are very rarely affected at the same time; that one follows the other at an interval even of years, and that the corresponding fingers on the ulnar or radial side—usually the ulnar—take much the same position in both hands, as is well illustrated in Figs. 4 and 5. These points are an argument in favor of the reflex action of one hand through a corresponding nerve to the opposite hand.

There was more or less swelling of the knuckles of both hands, resembling rheumatism. In 1889 I cut out the band in the right-hand finger, and made two transverse cuts to release the little finger. Six months later Dr. W. came to see about having the left hand cut, reporting that the operation on the right hand had been most satisfactory; that he had since had but little pain in the shoulder, and could put his arm forward without inducing pain. The left hand had been recently giving neuralgia of the ulnar side of the forearm and numbness of the little finger. Two years later he made the following graphic and convincing narrative of his pains which had been present at first:

1. Has had no knuckle swelling or suggestion of rheumatism.
2. There has been no pain at all in the right hand.
3. The neuralgia and pain in the right cervical region have not returned.
4. Until lately there has been no pain whatever in the right shoulder.
5. The burning of the left palm disappeared after the operation on the right hand, and has never returned.
6. The pain that was slight but present in the left arm before two years ago, no operation having been done here, has increased so as to be unpleasant, resembling sciatic neuralgia, not limited to any muscle, but distributed in the back of the arm and the ulnar side of the

forearm. The arm aches when raised to the horizontal, and has an unnatural feeling of heaviness.

7. Altogether, the left has been the bad arm this year, while before the operation on the right that one was the chief seat of trouble.

I now operated upon the left hand, and there has since been absolutely no return of trouble. The right hand illustrated how well the excision method of operation had done on the ring finger. The cicatrix was fine and free from recontraction. The little finger, on which cross-cuts were employed, recontracted somewhat.

A. B. was a car conductor, whose left index finger had drawn down into the palm and interfered seriously with

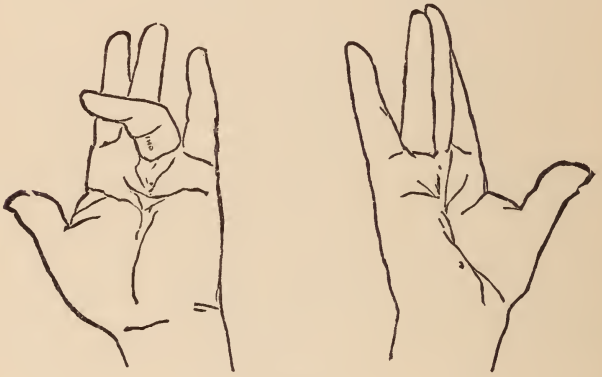

FIG. 4.

his work. He attributed it to former hard ploughing and hoeing. He had neuralgic rheumatism across the shoulders, so persistent that he got his coat on with difficulty. I made five transverse cuts and released the fingers perfectly. Two months later he reported himself absolutely well and free from pain, having worked since ten days after the operation.

In 1888 I operated on A. C., aged fifty-three years, for Dupuytren's contraction, which he attributed to a strain while turning a stopcock firmly some years before, when he heard a snap as if something was breaking in his palm. The palm puffed up, and then the swelling slowly disappeared, the palmar contraction following fifteen months afterward. The left showed a knot of contraction in

identically the same place as the opposite. Pain had commenced eight months before I saw him—a deep-seated neuralgic pain which made him think he had rheumatism. After the operation his pain disappeared.

Regarding Methods of Operation.—Following Mr. Adams' urgent advice, the tendency has been to resort to the subcutaneous division of the bands at several points in the palm. This method was uniformly resorted to by me in the early years of my experience. Finding many recontractions, and believing that the absolute safety of good surgical work enabled one to cut out and get permanently rid of the offending band of diseased fascia, I

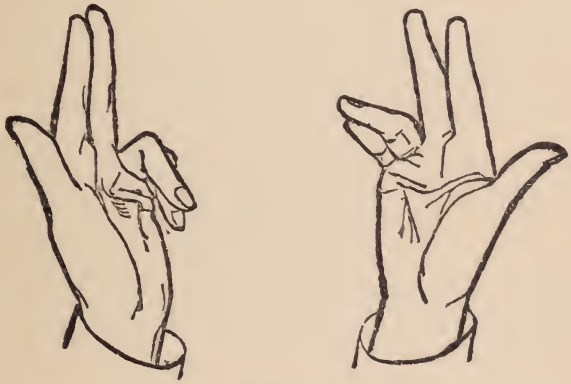

Fig. 5.

have during the last few years resorted in most cases to excision. The results have been better than by subcutaneous division in my hands. The operative procedure is thorough, safe, and exact. It can be done painlessly and bloodlessly with cocaine anæsthesia combined with Esmarch's bandage, and leaves soft linear scars.

The relative merits of excision and subcutaneous division have been illustrated by some of my cases in whom both methods were tried; for instance:

A lawyer from Indiana, whom I saw in 1887, had his right middle finger drawn down for twelve years, and the left became involved five years later. Three years

before I saw him he had had a cross-incision of the right-hand band in the palm, which brought the fingers straight, and it was useful for a time, though it afterward required another operation. This time the subcutaneous incisions were made by an able surgeon. It soon began to draw down again when the splint was removed. Six months later the left hand was operated upon by subcutaneous incisions. Contraction had been progressive ever since these wounds were healed. I operated by linear extirpation of the right-hand band, not exposing the tendon or nerves. On the left hand the cicatricial and fascial contractions had matted the tissues of the palms. I made cross-sections at seven points, making lozenge-shaped cuts when the finger was straightened. This left the finger free. The result was all that could be desired in both hands at the time. Three years later the left hand had undergone recontraction of the ring finger, which had been cross-cut, while the right, where excision of the band had been made, was straight and supple.

This proved incontrovertibly to my mind the superiority of excision. I then excised the band that had reformed in the left hand, which had become more prominent, and was able to release the fingers perfectly. Two years later a perfect result was shown from both operations. Five years had thus elapsed without recontraction of the first excised bands.

A tumefied condition of the fascial band often precedes the contracted stage, and can be readily cut across by open incision, after which the wound melts away invariably, though in a year or two the band is apt to recontract through the scar.

This dissolving of the tumefied cord when severed is as if, the nerve filaments being cut, the path of irritation is broken and the products of inflammation are quickly absorbed.

I now give decided preference to the cutting-out of contracted bands, which, though a somewhat delicate operation to do, is thorough and enduring in its results. Nowhere is the use of cocaine more satisfactory. Yet I have found, as has been observed also by others, that,

from some yet unexplained cause, cocaine will occasionally yield very slight anæsthesia. I have seen this on two or three occasions only. A fine hypodermic needle must be used, and a two-per-cent solution of cocaine. Not more than ten or twelve drops will be needed if properly placed in the derma at points one-third of an inch apart along the proposed incision over the band. There will always be found a layer of fat between the tendon sheath and fascia. Interrupted fine silk sutures should adapt the cut edges exactly.

A narrow strip of cleansed gutta-percha or protective is laid along the cut, and small compresses of damp gauze laid an inch deep over this. Over all a square of thin gutta-percha tissue will keep the dressing damp and favor the drainage of the capillary oozing from the cut. A firm bandage should be applied before the Esmarch bandage is taken off.

The completed operation and dressing is done within twenty minutes, which I have found to be about the limit of time that most patients can comfortably bear a snug Esmarch bandage.

The first dressing should be changed in twenty-four hours; the second on the fourth day, when stitches may be removed.

Contrary to the custom of Mr. Adams, who puts on a straight splint at once, I have found it better to let the fingers assume a comfortably correct position, under a dressing without splint (which will often induce pain by overstretching), and I put on no splint until a week has gone by and the parts are healed.

Although I have records in my private notebook of fifty cases of Dupuytren's contraction, representing about that number of operations, I have never seen a case of suppuration after operation, although most of those of the last four years have been excised.

The paralytic and inflammatory deformities of the hand —so-called "*main-en-griffe*"—are to be carefully distinguished from Dupuytren's contraction before operation,

the tendons in such cases being contracted, but buried in the flat palm and not raised like the bands of the latter.

Contractions simulating Dupuytren's.—Twice I have seen children with the little and ring fingers of each hand drawn down to the palm, looking like a Dupuytren's contraction, but purely of reflex origin. One case, seen five years ago, has lately been reported by the child's father as almost entirely straightened with time.

Another quite frequent deformity resembling a Dupuytren's contraction is seen in the little finger curved by reason of a congenitally short skin on its palmar side pre-

FIG. 6.

venting its being straightened out as its neighbors. On manipulating it one readily feels that there is no band under the skin.

I have seen one such finger with a double twist giving it a spiral curve toward its neighbor.

The palm is occasionally traversed by bands of congenitally short skin underlaid by somewhat limiting bands—not, however, of palmar fascia. This is well illustrated by the annexed Fig. 6, showing the hand of a young lady whose piano-playing was limited by inability to stretch the fingers far enough. This was greatly improved by several subcutaneous cuts.

To completely remedy these defects is not possible, but much may be done toward that end by cross-cuts of the short skin, allowing the lozenge-shaped gaps to granulate and be stretched later. This I have done in several cases and acquired a soft scar.

Neuroses of the Hand.—These belong mostly to the province of the neurologist, but some, being of surgical interest, may be mentioned:

An intelligent lady of forty-five years ran a threaded needle, head first, into the middle of her middle finger on its palmar side. She experienced not much pain, but an entirely disproportionate shock to her nervous system. She was bewildered and could not collect her thoughts for a few minutes. The needle being tightly stuck in, she had to use force and pulled it with her teeth. She observed a stringy fibre came with it, which her doctor said was a nerve. She walked home an hour later, but was overcome by bewildered feelings. She lost her memory and found herself leaning against a house some distance away.

Her nervousness continued, and on the second night she awoke like a mad person, with intense pain and a swollen finger. Suppuration, erysipelas, and pyæmia followed. One night she awoke with aphasia and left hemiplegia, and did not recover speech for six weeks.

When convalescing she had spasmodic contraction of the face, arms, and legs during dressing of the hand. The healthy side became hyperæsthetic. It was three months from the accident before she began to walk. She still mixed her words, and when I saw her two years later she was in an overwrought nervous state with a hand unfit for manual work. Memory, which had always been good before the accident, was still capricious. For example, she would forget in ten minutes all I had asked and said to her, but a week later it would recur clearly to her. She used wrong words and was not at the time conscious of it, but in half a minute realized she had spoken falsely.

When she began to speak after her sudden aphasia she had to learn over again the use of most words and the meaning of many. She still mixed up words in a sentence.

Such is the history of a striking case of shock to the central nervous system from a slight nerve injury of the hand.

A boy was brought to me with the ring and middle fingers of his hand half-closed into his palm. There was no Dupuytren's contraction of the fascia. He had been playing at jumping over posts some years before, and bent down the middle fingers, straining the knuckles, which enlarged, with subsequently some rigidity. Later the same pain and stiffness were reflected to the opposite hand with occasional neuralgia of the shoulders. He had flexor cramps when gripping a hammer, hoe, or rake.

Several cases narrated further on represent also neuroses from traumatisms.

Incidentally one may meet these conditions with hammer palsy, artisan's cramp, or musician's cramp, many of which I have seen. Among them, one more striking than another is that of a young lady, a violinist, who, being in perfect health, was assigned by her teacher to play a concerto in public. Time being limited, she practised one staccato movement, which was new to her, for two hours in succession, at the end of which time she broke down completely. Her hand was useless. There was complete muscular or nerve exhaustion. She was unable to write, or to use her violin. Later she had insufficiency of the eye muscles, requiring prisms, and flat-foot, requiring her to use crutches and have a metal arch put into her shoe.

Seven years have passed, and she has never regained nervous force enough to control her violin bow, or to write more than ten minutes without her hand giving out.

These few illustrative cases, chosen from many, suffice to show the intimate relation between many cases of hand troubles involving nerve ends and widespread disturbances of the system.

Tumors.—Of more special surgical interest are tumors of the hand and fingers, of which from many I will narrate three or four of special interest. It is in the hand that the smallest tumors are often the most troublesome,

because conspicuous. I have notes of one little tumor, as large as a small pea, on the end of the thumb in a woman who had been prevented from sewing by it, owing to pain on pressure with further pain in the arm. It was in and below the deep layer of the derma, and on being dissected out proved to be a purplish-looking spindle-celled sarcoma, denominated by Paget painful subcutaneous sarcoma. A second and larger one in another patient grew in the centre of the palm.

I have had four such tumors of the hand—two above the wrist—so painful on being struck that they caused the

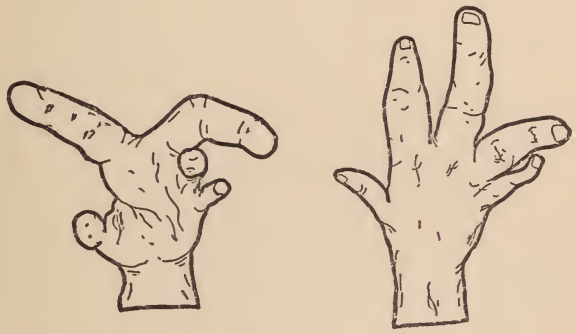

FIG. 7.

hand to drop anything that the patient might be carrying.

One which I dissected out ten years ago was a typical sarcoma, and had not recurred at the end of eight years. Nor have the others returned, as far as I know. Another tumor at the base of the index was as large as a walnut, was soft enough to give a striking sense of fluctuation, but proved to be a pure fibroma with soft stroma.

In the palm one occasionally sees small herniæ of the sheaths of the tendons buried in the fat of the palm, little cysts filled with transparent jelly and with very small connection with the sheath. These are like weeping sinews, so common on the back of the wrist, but cannot be dissipated by a blow. I have seen them disappear

without treatment, but this is so rare that one should dissect them out if they give trouble. Such a one I recently dissected out from the fat of the palm at the root of the ring finger, where it had given annoyance.

Hypertrophy of the Fingers.—Hypertrophy of the knuckles, very well described in Tillmann's "Surgery" (Leipzig, 1892), is an epiphyseal hyperostosis, with elongation as well as broadening of the bones of some of the fin-

FIG. 8.

FIG. 9.

gers. Others on the same hand often remain normal. The drawings (Fig. 7) are taken from a photograph of a lad sent to me by a physician in Maine. The great toe and the second toe showed the same extraordinary enlargement.

Trigger Finger.—I fancy it may be such a growth that causes the curious trouble denominated (trigger) "snap finger," of which I reported five cases, with illustrations, some years since (see Figs. 8, 9, 10, and 11, here published

by permission of the *Medical News*, in which they originally appeared). The patient closes all his fingers in the palm, and on opening them finds that one will stay shut and can only be opened by using the other hand to effect it, when it flies open, like a knife blade, with a snap.

It is said to be due to a tumefied condition of a point of the tendon, making a bulbous enlargement that catches under the ligament at the base of the finger. One such case is reported as being capable of dissection and relief. In my cases there was no perceptible swelling, and all

FIG. 10.

recovered after a few months, part of which time they were kept on a little wood splint with pad pressure, which prevented friction and irritation.

Of particular importance I will mention two deformities of the end joint of the fingers, happening from slight accident, which need the earliest surgical care and admit of most particular work.

Drop Finger.—Two cases illustrate what I would call "drop finger." Both happened from apparently insignificant causes:

In one a lady was taking off a stocking, and, pushing it down the side of her leg with the tips of her fingers, suddenly found the end joint of her ring finger had given way and hung at right angles to the finger, powerless. With her other hand she could straighten it, but was unable to support it. It appeared to have nothing but skin over the joint to hold it up. The extensor tendon, where it thins out disproportionally to its size above, had torn away from its delicate attachment to the base of the last joint.

In a fortnight the joint became red and tender; a sharp, shooting pain extended from the knuckle to the wrist,

FIG. 11.

FIG. 12.

FIG. 13.

forearm, arm, shoulder, and back of the neck. It was often severe enough to make her wholly sick.

She volunteered the statement that, a fortnight after the accident, the same joint of the other hand pained her when using it. This pain lingered six months after, when I first saw her. The injured hand was quite disabled on that account. It was tender if touched, ached if used, and if struck by mishap it "made her sick all over."

It was now impossible to straighten it on account of the inflammatory sealing up of the torn capsule. I therefore resected the joint and made a solid, straight, and useful

finger. When it was healed she was free from pain. I have to-day seen her, more than five years since operation, and she has not since had pain and uses the finger as if it were never hurt.

The second case in which I operated was on a prominent architect, whose ring finger dropped useless at the last joint from the slight pressure of his finger tips pushing across a paper from off which he was brushing some crumbs.

I operated two weeks later, when his physician found himself unable to keep the joint straight, even using a splint. Through a linear cut on the back of the knuckle I sutured the torn end of the tendon to the periosteum of the base of the last joint. The result four years after has been most admirable.

Baseball Finger.—The reverse deformity to the above is now very commonly seen in baseball players. The last joint is violently dislocated backward and cannot be replaced, on account of the flexor tendons wrapping themselves round the head of the proximal bone of the joint, which also slips out through a buttonhole of the capsule. This accident makes a permanent bayonet deformity. It is very apt to be compounded by laceration of the skin at the flexure crease. In three such cases I have been able to restore the parts perfectly through incision, though in one I had to resect the head of the bone and make a stiff joint.

Burn Scars.—Some of the worst deformities of the hand one ever sees are produced by burns. These have heretofore been the *bête-noire* of the surgeon. Now it is possible, thanks to Prof. Thiersch, to restore many of the worst cases to usefulness by skin-grafting.

I will mention only a few of the bad cases I have had.

Two almost similar in deformity were produced in girls working in laundries where ironing mangles were used, the upper cylinder of which is kept hot.

Their hands, being drawn under the roller, were burned to the bone from the knuckles to the wrist, and the resulting slough, including tendons, left a scar which drew the hand directly back upon the wrist.

Dissecting out such massive scars, one brings the fingers as nearly straight as possible and makes Thiersch grafts over the parts exposed. Two such cases are illustrated on the charts.

A unique accident brought a young man to me with broad ulcerated surfaces around each wrist, threatening him with ultimate loss of wrist action. He had been cleaning a pair of gloves with benzine and had on celluloid cuffs. The former ignited and set fire to his cuffs, which burned like tinder. He made frantic efforts to get them off, but they were stiff and only burned him worse for handling, so that they practically burned themselves off *in situ*.

I planted Thiersch grafts on both wrists with the happiest result. All scars were perfectly supple six months afterward.

Indeed, I may say that these grafts do not show the contracting tendency that the scar tissue does, and are adapted to burn cicatrices perfectly. Indeed, I have covered the entire arm from the back of the hand to above the elbow with them, and had fine supple skin three years after.

Webbed Fingers.—There is one finger deformity to which this method, therefore, specially applies—namely, webbed fingers. All that is needful is to split the web and dissect out any tough part well down between the fingers, then put a long strip of Thiersch graft up one side and down the other, retaining it by gutta-percha tissue at the flexion.

Such a case of all the fingers I put up in a permanent dressing at the time of operation and removed at three weeks, to find the parts perfectly healed.

Conservative Surgery.—I will not weary you with enumerating further the many hand troubles which always elicit the surgeon's pity as well as skill, but will venture afield into a novel inquiry that has occupied some of my thoughts of late.

Last spring a well-to-do man from the West was sent to me with both hands gone above the wrists. A dyna-

mite cartridge, with which he had been expecting to do some fishing by a method of which the law takes cognizance in some States, had exploded in his hands and amputation had been required. With Western energy he had come East to see if there was not some one who could graft a new hand upon his arm. He said he was sure he could persuade the Territorial Governor to release a convict who would sacrifice a hand for remuneration, which he himself was ready to pay. I could but smile at the Munchausen tale such a deed would make, and took his address, promising, if ever it became possible, I would communicate with him.

Not long after a man came to me from New Jersey

FIG. 14.

FIG. 15.

who had had a buzz saw plough through his hand from the back, severing all the fingers and the thumb through their metacarpal knuckles, cutting obliquely toward the wrist, and leaving the hand hanging by the skin at the line of the palmar arch. Every tendon—both extensors and flexors, except the flexor of the thumb—was cut across.

It seemed inevitable the parts must be sacrificed, but I decided to see how much could be done, and spent three hours and a half trimming the torn tendons and muscles, resecting the disrupted joints, and suturing the whole. I can hardly believe that the skin and some soft parts alone nourished the fingers, but that one or more of the

digital arteries were probably spared, though I did not discover them.

The little finger was the only one that became gangrenous, the thumb and three fingers showing good vitality.

I saw the man a few days since, after six months. He now uses his hand for all work, and has a little flexion of the fingers and slight independent flexion of the end joints. The thumb grasps tightly against all the fingers, and he writes almost as well as ever.

In view of the fact that surgeons have replaced a finger, an end of a nose, and small parts of flesh under favorable conditions, I asked myself *why not* a major part, such as a hand or a leg? Is it our inability to nourish the part, or to innervate it, or to unite tendons, muscles, or bone? The latter surely gives no trouble. Tendons are sutured every day, and under favorable circumstances their function is restored. Nerves are frequently sutured and functional restoration is often recorded.

Is it impossible to restore an arterial supply once cut off? Veins are frequently cut and sutured, and perform perfectly afterward. Is there no way to restore an artery?

The question seems to narrow itself down to nourishing a limb in order to restore it.

Experiments.—During the summer I made a few experiments at the Carnegie Laboratory, with the assistance of Dr. Theodore Dunham and through the kindness of Prof. E. K. Dunham.

To see whether a thin glass tube would be tolerated in a sterilized state within an artery, I made a number of half-inch pieces to suit the calibre of a dog's femoral, constricted them very slightly to an hour-glass shape, and smoothed their ends by heat, so that no surface roughness should induce clotting. Cutting the femorals across, I tied each end over the tube by a fine silk thread and tied the thread ends together. Primary union resulted, and the dog's legs are as good as ever.

I feared, however, that the artery might have become blocked in this case, and the collateral circulation had

nourished the limb. I therefore cut one out to see, and tied the femoral above and below. The tube was free in a dilated end of the artery, and slow endarteritis had sealed it below. Whether this would necessarily occur I decided to see by putting a tube in the aorta, where it seems as if collateral circulation could not save the limbs. I chose a cat, whose abdomen I opened and whose aorta I cut across, clamping lightly above and below. Into this I tied an inch of very thin glass tube sterilized by boiling, and filled with water just before inserting into the lower end, so as not to have air emboli. The cat made a perfect recovery, and after four months I show you him to-night, fat and strong, with a glass tube in his aorta.

I afterward tried the same experiment on a large sheep, whose aorta is thicker-walled and larger. Unfortunately, I clamped the aorta so tightly by a broad pedicle clamp that I crushed the inner wall, and at autopsy, two days afterward, the site of clamping was blocked by adhesive clot.

Two days ago Dr. Dunham and Dr. Cushman repeated the experiment on a large dog, and he has thus far been well.

Four months ago I added one step more to the experiments. I dissected out the brachial artery and vein near the axilla of a dog's forelimb, and, holding these apart, amputated the limb through the shoulder muscles and sawed through the bone, leaving the limb attached only by the vessels. I then sutured the bone with a silver wire and the nerves with fine silk. Each muscle I sutured by itself with catgut, making a separate series of continuous suturing of the fascia lata and skin.

The leg was then enveloped in sterilized dressing, a liberal use of iodoform gauze being the essential part. Over all, cotton and a plaster jacket were placed, leaving him three legs to walk on. A small drain of the axilla was removed at the first dressing and a permanent dressing applied, which remained two months. The dog's leg united perfectly, and he is here to show how well he can use it.

Thus we see that, if in an amputated limb an artery can be left, the limb will survive the division of everything else. And, further, it may be asserted that, if an arterial supply can be restored to a completely amputated limb, *that* limb also may be grafted back to its original or a corresponding stump.

Up to the present I do not feel that we have incontrovertibly proved that arterial continuity can be restored by a glass tube in all cases. But it is not impossible that, if slow proliferating endarteritis shuts up the main artery in a few weeks, the anastomosis through the soft cicatrix of vascular muscle and skin may be able, by the recuperative power of Nature, to take up the nutrition and ultimately carry it on.

The final experiment of the series which I set out to make—namely, the complete amputation of an animal's limb and its restoration—requires preparation, assistance, and time, so that I have not been able to complete it within the period preceding the time appointed to read this paper. I shall therefore report at another date to the Academy such facts as may be further developed.

Some one may ask, Where is the supply of limbs to come from, should it ever be possible to graft a leg or an arm? I may say that I doubt not that a limb crushed, let us say, at the thigh or shoulder, requiring amputation, would admit of Esmarch's bandage being applied to expel its blood and of being used after amputation. It should be just as viable as any limb which we keep bloodless for hours under Esmarch's bandage and have no trouble with when blood is let into it. Why not another man's blood as well as its owner's?

I do not expect that this vision of surgical possibilities will be realized soon, nor do I think enough has been proved to warrant much hope, but I feel that experiment in that direction will yield much surgical instruction.

The tolerance of sterilized glass tubes in the larger arteries admits of further application than has been hinted at in this paper.

SCIENTIFIC WORK

IN THE

SECTIONS OF THE ACADEMY.

JANUARY 17TH, 1893.—Discussion: "The relations of the bacterial theory of disease to practical medicine."

J. D. Byron, M.D. Paper: "The Accepted Facts."

W. Gilman Thompson, M.D. Paper: "Deductions from physiological relations."

H. F. Walker, M.D., and A. H. Smith, M.D. Paper: "Effects on individual practitioners."

A. L. Loomis, M.D. Paper: "Effect on the profession at large."

W. H. Thomson, M.D. Paper: "Effect on therapeutics and materia medica."

General discussion: S. Baruch, M.D.

FEBRUARY 21ST, 1893.—G. B. Fowler, M.D. Paper: "Diet in disease." Not published. Discussion: Drs. H. D. Chapin, W. H. Porter, W. W. Van Valzah, C. E. Quimby.

APRIL 18TH, 1893.—W. H. Porter, M.D. Paper: "Hæmogallol in anæmia of malassimilation." Published, Amer. Med.-Surg. Bull., 1893, vi., 482.

S. Baruch, M.D. Paper: "Some non-medicinal remedies in anæmia." Published, N. Y. Med. Jour., 1893, lvii., 522.

T. S. Southworth, M.D. Paper: "The clinical study of the blood in anæmia." Exhibition of microscopic specimens of normal and anæmic blood. Published, N. Y. Med. Jour., 1893, lviii., 624.

H. P. Loomis, M.D., exhibited a specimen of peptomanganate of iron, and gave clinical histories of sixteen cases in which it had been tested.

C. E. Quimby, M.D., described the hæmatokeit, and suggested certain modifications in its construction that would diminish its size and increase its precision.

MAY 16TH, 1893.—A. D. Rockwell, M.D. Paper: "Sexual erethism: its neurotic origin, and treatment." Published, N. Y. Med. Jour., 1893, lviii., 201.

R. W. Wilcox, M.D. Paper: "Asaprol, a new antiseptic." Published, Epitome of Med., 1893, x., 289.

J. K. Crook, M.D. Paper: "The hydrochlorsulphate of quinine: a new soluble salt," with exhibition of the drug. Published, Amer. Med.-Surg. Bull., 1893, vi., 723.

OCTOBER 17TH, 1893.—W. B. Vanderpoel, M.D. Paper: "Albuminuria without manifest organic renal lesions." Published, Med. Rec., 1893, xliv., 611. Discussion: Drs. A. L. Loomis, W. H. Porter, R. C. M. Page, G. B. Fowler, L. Waldstein.

C. E. Quimby, M.D., asked to what extent nitrogenous food should be permitted in renal disease. This was discussed by Drs. Fowler, Porter, and Waldstein.

NOVEMBER 21ST, 1893.—W. L. Stowell, M.D. Paper: "Changes in therapeutics in recent years." Published, Amer. Med.-Surg. Bull., 1894, vii., 779. Discussion: Drs. W. H. Draper, A. A. Smith, G. L. Peabody.

DECEMBER 26TH, 1893.—Discussion: "The specific and relative value of the important factors of longevity." Discussion: Drs. E. W. Lambert, B. Symonds, O. H. Rogers, M. Morris, C. E. Quimby.

E. LE FEVRE, M.D.,
Secretary.

SECTION ON GENERAL SURGERY.

JANUARY 9TH, 1893.—*Cases presented:*

1. A. M. Phelps, M.D. Removal of large neoplasm of the cheek, with subsequent flap operation.
2. H. Lilienthal, M.D. Epitrochlear gland enlargement following contused wound of the proximal knuckle of the middle finger.
3. J. D. Bryant, M.D. Recovery from large iliac

aneurism treated by Macewen's method of needling.

4. C. Beck, M.D. Tuberculosis of the peritoneum in a child, cured by abdominal incision.

W. Meyer, M.D. Paper: "Incision of retropharyngeal abscess, according to antiseptic principles, from the neck." Not published. Discussion: Drs. R. Whitman, A. M. Phelps, R. H. Sayre, B. F. Curtis, C. Beck, H. Lilienthal, W. W. Van Arsdale, and W. Meyer.

H. Lilienthal, M.D. Paper: "The site of infection producing epitrochlear glandular enlargement." Published, Med. Rec., 1894, xlv., 101. Discussion: Drs. H. Goldenberg and H. Lilienthal.

FEBRUARY 13TH, 1893.—*Cases presented:*

1. J. A. Wyeth, M.D. A case of resection of the intestine for occlusion caused by a mass of lumbricoid worms.
2. F. Kammerer, M.D. A case of resection of the hip joint for old dislocation.
3. W. Meyer, M.D. A case illustrating Busch's modification of the Indian method of rhinoplasty; also a case of meloplasty for large epithelioma of the nose and face.
4. R. H. Sayre, M.D. A case of club-hand with absence of a part of the carpus and of the radius.

Pathological Specimens:

1. B. F. Curtis, M.D. Littré's hernia.
2. B. F. Curtis, M.D. Specimen of sarcoma of the leg.
3. F. Lange, M.D. Specimen of gangrenous hernia, and gangrenous line of suture after the operation of lateral anastomosis for its relief.
4. F. Lange, M.D. Specimen of large intestine, showing union by circular suture after removal of malignant tumor.
5. F. Kammerer, M.D. Specimen of ruptured bladder.

6. J. A. Wyeth, M.D., for Dr. Holson. Specimen of sarcoma of the sigmoid flexure.

MARCH 13TH, 1893.—*Cases presented:*

1. H. Lilienthal, M.D. Congenital cystic tumor of the coccygeal region.
2. B. F. Curtis, M.D. A patient operated on for osteomyelitic abscess of the humerus, with subsequent implantation of decalcified bone.
3. C. N. Dowd, M.D. A patient showing the result of Auger's operation for ingrowing toenail.

C. N. Dowd, M.D. Paper: "A report of twenty-three cases of ingrowing toenail operated upon by the method of Auger." Published, Med. Rec., 1893, xliv., 289. Discussion: Drs. R. H. Sayre, F. Kammerer, W. Meyer, B. F. Curtis, H. Lilienthal, J. D. Bryant, and C. N. Dowd.

B. F. Curtis, M.D. Paper: "Bone transplantation and implantation." Published, Amer. Jour. Med. Sci., 1893, cvi., 30. Discussion: Drs. T. H. Manley, R. T. Morris, S. Lloyd, F. Kammerer, W. Meyer, C. N. D. Jones, J. D. Bryant, W. B. Coley, and B. F. Curtis.

APRIL 10TH, 1893.—*Cases presented:*

1. W. B. Coley, M.D. A case of strangulated inguinal hernia in a child of eight months. Cured by operation.
2. W. B. Coley, M.D. A case of strangulated inguinal hernia in a child, with persistent high temperature, 107°, before and for some time after operation for its relief.

R. T. Morris, M.D. Paper: "Infectious appendicitis 'ecphyaditis,' accompanied by microphotographs, charts, and pathological specimens." Published, Ann. Surg., 1893, xviii., 365.

MAY 8TH, 1893.—*Cases presented:*

1. C. N. Dowd, M.D. A case of sarcoma of the leg.
2. C. K. Briddon, M.D. A case of hysterectomy for cancer by Schede's method.

3. C. A. Powers, M.D. Dislocation of the lower jaw; reposition after one hundred and twenty days.
4. C. A. Powers, M.D. Presentation of pathological specimens. Periosteal sarcoma of the leg.
5. C. N. Dowd, M.D. Apparatus for sterilizing catgut.

JUNE 12TH, 1893.—*Cases presented:*

1. T. R. Savage, M.D. A case of spina bifida cured by operation at the age of four weeks.
2. C. Beck, M.D. Resection of the hip joint in a child to demonstrate the advantages of the open method of treatment.

W. W. Van Arsdale, M.D. Paper: "The treatment of granulating wounds." Published, N. Y. Med. Jour., 1893, lviii., 115. Discussion: Drs. Armstrong, Girdner, Bodine, Gallant, Townsend, Agramonte, Jones, Dowd, and Van Arsdale.

Pathological Specimens:

C. N. Dowd, M.D. 1. Two specimens of sarcoma of the leg.

2. Specimen of perforation of the duodenum, the symptoms of which had simulated appendicitis.

OCTOBER 9TH, 1893.—*Cases presented:*

Two patients illustrating implantation of foreign substances.

1. R. F. Weir, M.D. A celluloid plate to cover a defect in the skull.
2. R. F. Weir, M.D. A celluloid ball implanted in the scrotum in the place of a testicle removed for tuberculous disease.
3. R. F. Weir, M.D. Also two cases operated on two years before for the radical cure of inguinal hernia by Bassini's method; cured.
4. C. Beck, M.D. A case of sarcoma of the arm cured by operation.
5. C. Beck, M.D. A case illustrating cure of tuberculous peritonitis by laparotomy, previously shown at the January meeting.

Pathological Specimens:

1. R. F. Weir, M.D. Tuberculous testicles and seminal vesicles removed from the patient shown earlier in the evening, by Roux's method.
2. C. A. Powers, M.D. Dermoid cyst removed from the sacral region.

NOVEMBER 13TH, 1893.—The general meeting of the Academy was in charge of the Surgical Section.

J. B. Murphy, M.D., of Chicago, read the paper, on "Intestinal Surgery."

W. W. VAN ARSDALE, M.D.,
Secretary.

SECTION ON GENITO-URINARY SURGERY.

JANUARY 10TH, 1893.—S. Alexander, M.D. Paper: "Some of the mistakes in using intravesical injections in the cystitis of prostatics." Published, Jour. Cut. and Gen.-Urin. Dis., 1893, xi., 150.

FEBRUARY 14TH, 1893.—W. K. Otis, M.D. Paper: "The treatment of suppurating buboes by injections of iodoform ointment." Published, Jour. Cut. and Gen.-Urin. Dis., 1893, xi., 174.

MARCH 14TH, 1893.—G. K. Swinburne, M.D. Paper: "A case of syphilitic reinfection." Not published.

G. E. Brewer, M.D. Paper: "A few cases of non-specific urethritis." Not published.

APRIL 11TH, 1893.—H. Koplik, M.D. Paper: "Urogenital blennorrhœa in children; Vulvo-vaginitis in girls; Urethritis in boys." Published, Jour. Cut. and Gen.-Urin. Dis., 1893, xi., 210, 263.

MAY 9TH, 1893.—H. G. Klotz, M.D. Paper: "On the occurrence of tertiary lesions of syphilis as the result of direct local infection, with general remarks on syphilis as an infectious disease." Published, Jour. Cut. and Gen.-Urin. Dis., 1893, xi., 276 et seq.

E. Fuller, M.D. Paper: "Seminal vesiculitis." Published, Jour. Cut. and Gen.-Urin. Dis., 1893, xi., 333.

R. Guiteras, M.D. Paper: "Gonorrhœal rheuma-

tism and its treatment." Published, N. Y. Med. Jour., 1894, lix., 355.

NOVEMBER 14TH, 1893.—W. K. Otis, M.D. Paper: "Anourethroscopy." Not published.

DECEMBER 12TH, 1893.—R. W. Taylor, M.D. Paper: "Some unusual new growths of the vulva." Published, Am. Jour. Med. Sci., 1894, cvii., 52.

E. Fuller, M.D. Paper: "The requisites of a suspensory bandage, including remarks on its uses and abuses." Published, Jour. Cut. and Gen.-Urin. Dis., 1894, xii., 45.

J. R. HAYDEN, M.D.,
Secretary.

SECTION ON ORTHOPÆDIC SURGERY.

JANUARY 20TH, 1893.—*Presentation of cases:*

R. H. Sayre, M.D. Spastic paraplegia, illustrating contractured tendon and the effect of point pressure. Discussion: Drs. Peterson, Shaffer, Taylor, and Sayre.

T. H. Myers, M.D. Paper: "The paralysis of the sphincters in Pott's disease." Not published.

R. H. Sayre, M.D. Paper: "Pott's paraplegia." Not published. Discussion: Drs. Shaffer, Berg, Whitman, Peterson, Lloyd, Collins, Ketch, Churchill, Galbraith, Taylor, Myers, and Sayre.

FEBRUARY 17TH, 1893.—*Presentation of cases:*

S. Ketch, M.D. Congenital flat-foot due to abnormal relaxation of the ligaments of the tarsus in common with general ligamentous relaxation over the whole body; and also a case of pes cavus following anterior poliomyelitis.

N. M. Shaffer, M.D., also showed a number of cases of flat-foot.

N. M. Shaffer, M.D. Paper: "Flat-foot: its cause and treatment." Not published. Discussion: Drs. Whitman, Gibney, Ketch, Hubbard, Sayre, and Shaffer.

March 17th, 1893.—H. L. Taylor, M.D. Paper: "A cardinal point in the management of abscess complicating joint diseases." Not published. Discussion: Drs. Judson, Hubbard, Sayre, Jones, and Townsend.

April 21st, 1893.—*Presentation of cases:*

R. Whitman, M.D. Patient with abnormally long and prehensile large toes on both feet.

W. Meyer, M.D. Result of nailing the head to the neck in intracapsular fracture of the left femur, with excellent functional result.

V. P. Gibney, M.D. Result of Hoffa's operation for congenital dislocation of the femur.

V. P. Gibney, M.D. History of a case of congenital dislocation of the femur operated upon by the Lorenz method.

R. Whitman, M.D. Paper: "Elements of the differential diagnosis of Pott's disease in childhood." Published, N. Y. Polyclinic, 1893, i., 123. Discussion: Drs. Judson, Ketch, Phelps, Myers, Sayre, and Townsend.

May 19th, 1893.—*Presentation of cases:*

T. H. Myers, M.D. Paralytic calcaneo-valgus treated by removal of wedge from astragalus behind the astragalo-tibial joint.

S. Ketch, M.D. Marked varus of one foot of a neuro-mimetic variety, improved by suggestion.

S. Ketch, M.D. Flat-foot, one side treated by operation, the other mechanically.

J. E. Goldthwaite, M.D., of Boston. Paper: "The forcible correction of angular deformities of the knee by mechanical apparatus." Published, Bost. Med. Surg. Jour., 1893, cxxix., 242.

Presentation of cases:

T. H. Myers, M.D. Plaster of Paris splint, with a spring for correcting angular deformity at the knee.

Presentation of instrument:

R. H. Sayre, M.D. Robin's osteoclast and its use for

correcting angular deformities of the knee. Discussion: Drs. Berg, Taylor, Ketch, and Whitman.

OCTOBER 20TH, 1893.—*Presentation of cases:*

R. H. Sayre, M.D. Congenital club-hand.

V. P. Gibney, M.D. Myositis ossificans, and history of second case. Discussion: Drs. Shaffer, Hubbard, and Gibney.

A. R. Shands, M.D., of Washington, D.C. Paper: "The treatment of club-foot by Wolff's method." Published, Virg. Med. Month., 1894, 5, xxi., 504 Discussion: Drs. Shaffer, Taylor, and Judson.

NOVEMBER 17TH, 1893.—*Presentation of cases:*

R. Whitman, M.D. Induration of the sterno-mastoid.

S. Ketch, M.D. Pott's disease with unusual symptoms.

S. Ketch, M.D. Pott's disease in an adult with a very long curve.

N. M. Shaffer, M.D. Relapse after excision of the hip (two cases).

W. R. Townsend, M.D. Lateral curvature due to rickets.

T. H. Myers, M.D. Deformity of the great toes accompanied with flat-foot.

DECEMBER 15TH, 1893.—*Presentation of cases:*

R. Whitman, M.D. Hæmatoma of the sterno-mastoid with congenital torticollis, shortened sterno-mastoid, facial hemi-atrophy, and club-foot.

R. Whitman, M.D. Congenital torticollis following delivery with forceps.

R. Whitman, M.D. Pott's disease in which paralysis antedated deformity.

R. Whitman, M.D. Pott's disease with psoas contraction of two years' standing.

V. P. Gibney, M.D. Hip disease treated by the Lorenz method.

V. P. Gibney, M.D. Excision of hip joint (two cases).

A. M. Phelps, M.D. Excision of tarsus with reproduction of bone by blood clot.

C. Beck, M.D. Laminectomy for Pott's paraplegia. Published, Amer. Med.-Surg. Bull., 1894, vii., 145.

N. M. Shaffer, M.D. Paper: "New operation for the relief of rotary lateral curvature." Published, Amer. Med.-Surg. Jour., 1894, vii., 51. Discussion: Drs. Haines, Myers, Sayre, Ketch, Lloyd, and Shaffer.

R. H. SAYRE, M.D.,
Secretary.

SECTION ON OPHTHALMOLOGY AND OTOLOGY.

JANUARY 16TH, 1893.—Chas. H. May, M.D. Paper: "A peculiar form of retinitis." Not published.

W. A. Holden, M.D. Paper: "Cases of embolism of a branch of the central artery of the retina." Published, Arch. Ophthalmol., 1893, xxii., 90.

H. W. Schwarzschild, M.D. Paper: "Case of mucous patch of the conjunctiva, complicated with a pseudomembrane." Published, Med. Rec., 1893, xliii., 490.

FEBRUARY 22D, 1893.—E. B. Dench, M.D., exhibited patients from whom he had removed the membrana tympani, malleus, and incus for impaired hearing due to chronic otitis media.

E. Gruening, M.D., presented a case of sarcoma of the lower lid of right eye.

W. A. Vulpius, M.D., presented a case of syphilide of external auditory canal; also case of fracture through petrous portion of temporal bone.

W. E. Lambert, M.D., presented a case of colloid deposit in choroid.

C. Koller, M.D., presented a series of clinical cases.

MARCH 20TH, 1893.—T. R. Pooley, M.D., presented a case of epithelioma of lower lid.

E. Gruening, M.D., gave history of a piece of steel embedded in the iris.

H. Knapp, M.D. Paper: "The early diagnosis of sarcoma of the choroid." Not published.

D. Webster, M.D., reported a peculiar case of cataract.

The Section then discussed the "Kindergarten system in the public schools, and its effect on the eyes of young children."

APRIL 7TH, 1893.—F. W. Ring, M.D., presented an ocular mask.

S. McA. Payne, M.D., presented various instruments.

A. Schapringer, M.D. Paper: "The illusion known as 'fluttering heart.'"

H. D. Noyes, M.D. Paper: "Mode of management of immature cataracts."

MAY 15TH, 1893.—D. Webster, M.D., reported a case of lamellar cataract.

W. B. Marple, M.D. Paper: "The pathology of hypopyon keratitis." Published, Arch. Ophthalmol., 1893, xxii., 425.

S. McA. Payne, M.D. Paper: "Insufficiency of the ocular muscles due to errors of refraction." Published, N. Y. Polyclinic, 1893, ii., 6.

OCTOBER 16TH, 1893.—J. A. Andrews, M.D., presented an apparatus for sterilizing instruments.

T. R. Pooley, M.D. Paper: "A contribution to the brain surgery of ear disease." Published, N. Y. Polyclinic, 1893, ii., 97.

A. Schapringer, M.D. Paper: "The proximate cause of the transient form of myopia associated with iritis." Published, N. Y. Med. Jour., 1893, lviii., 465.

NOVEMBER 19TH, 1893.—W. B. Johnson, M.D., reported a case of amblyopia ex anopsia.

J. E. Weeks, M.D., reported a case of sympathetic ophthalmia occurring many years after injury to other eye.

H. Knapp, M.D., presented an orbital cyst.

C. Heitzmann, M.D., demonstrated sections of cat's cornea.

F. Valk, M.D. Paper: "The power of the intimi." Published, Med. Rec., 1894, xlvi., 70.

DECEMBER 18TH, 1893.—W. M. Leszynsky, M.D., reported a case of double optic neuritis.

F. Van Fleet, M.D. Paper: "Astigmatism and the ophthalmometer." Published, Arch. Ophthalmol., 1894, xxiii., No. 1.

W. B. MARPLE, M.D.,
Secretary.

SECTION ON LARYNGOLOGY.

JANUARY 25TH, 1893.—J. E. Nichols, M.D., presented a patient with sinus of the frontal bone due to traumatism; also a woman suffering from an abscess in the region of the eye, causing considerable exophthalmos.

D. B. Delavan, M.D., presented a patient who had previously been operated on for sarcoma of the nasal cavity. Case discussed by M. D. Lederman, M.D.

A. Rupp, M.D., presented a skull showing an anomalous condition of the accessory sinuses. Discussed by W. C. Phillips, M.D.

R. C. Myles, M.D. Paper: "Diseases of the accessory sinuses: treatment and indications for operation." Not published. Discussion: Drs. Harrison Allen (Philadelphia) and J. H. Bryan (Washington).

A. C. Getchell, M.D., Worcester, Mass., read the history of a case of malignant disease of the antrum.

FEBRUARY 22D, 1893.—W. K. Simpson, M.D., presented a case of enlarged tonsils occurring in the course of secondary syphilis. Discussed by J. E. Newcomb, M.D.

G. A. Richards, M.D. Paper: "Idiopathic, gangrenous gingivitis in adults." Published, Med. Rec., 1893, xliii., 457. Discussed by W. H. Park, M.D., who gave the history of a case of noma recently seen by him.

Conclusion of discussion on paper of Dr. Myles, read at January meeting. Subject discussed by Drs. Gleitsmann, Phillips, Freudenthal, Knight, Simpson, and Richards.

MARCH 22D, 1893.—T. P. Berens, M.D., presented a case for diagnosis. Discussion: Drs. Wright, Nichols, Myles, Swain (New Haven), and Knight. All agreed in the diagnosis of pachydermia of the interarytenoid space.

C. H. Knight, M.D., presented a patient (boy) upon whom he had recently operated for enlarged tonsils.

W. C. Phillips, M.D., gave the history of a case of purpura hæmorrhagica in which tracheotomy was performed.

F. E. Hopkins, M.D., presented a patient suffering from pulmonary tuberculosis in whom a neoplasm had been removed from the left vocal cord. Discussion: Drs. Wright and Park.

W. H. Park, M.D., presented a patient with laryngeal neoplasm—probably a fibroma.

J. Wright, M.D., related the history of a case of laryngeal fibrous epithelioma. Discussion: Drs. Knight and Swain.

APRIL 26TH, 1893.—C. H. Knight, M.D., presented a case of cyst of the epiglottis.

H. H. Butts, M.D. Paper: "A new instrument for controlling tonsillar hæmorrhage." Published, Med. Rec., 1893, xliv., 11. Discussion: Dr. Knight.

C. H. Knight, M.D. "Notes of a case of empyema of the antrum illustrating (1) the necessity of cleaning out the middle meatus and (2) the resistance of the mucous membrane to cocaine anæsthesia." Published, Med. Week., 1893, i., 232. Discussion: Dr. Newcomb.

MAY 24TH, 1893.—F. E. Hopkins, M.D., reported "An unusual case of tuberculosis of the larynx." Pub-

lished, Manhattan Eye and Ear Hosp. Report, vol. i., 1894.

W. Freudenthal, M.D. Paper: "Internal massage in diseases of the nose and throat." Published, Med. Rec., 1893, xliv., 97.

J. S. Crossfield, M.D., Hartford, Conn. Paper: "Tuberculosis of the larynx and pharynx." Published, Med. Rec., 1894, xlvi., 411. Discussion: Drs. Delavan and Rice.

OCTOBER 25TH, 1893.—J. E. Nichols, M.D., presented a case of hypertrophy of the mucous membrane covering the vomer posteriorly.

H. Knapp, M.D., exhibited a fresh post-mortem specimen from the skull of a patient who had died from the extension of ethmoidal and sphenoidal cell disease. Discussion: Drs. Bosworth and Lincoln.

J. W. Gleitsmann, M.D., exhibited a patient on whom he had operated for disease of the sphenoidal cells.

C. H. Knight, M.D., exhibited a patient with recurrent papillomata of the larynx.

N. S. Roberts, M.D. Paper: "Adenoids in young infants." Published, N. Y. Eye and Ear Infirm. Reports, vol. ii., part 1, 1894. Discussion: Drs. Dench, Delavan, Nichols, Phillips, Park, and Lincoln.

NOVEMBER 22D, 1893.—M. J. Asch, M.D. Paper: "Report of a case of endothelioma of the tonsil." Published, N. Y. Eye and Ear Infirm. Reports, vol. ii., part 1, 1894.

W. F. Chappell, M.D., exhibited a case of lymphosarcoma of the tonsil. Discussion: Drs. Quinlan and Coleman.

J. E. Nichols, M.D., exhibited cases of syphilitic adhesions of the soft palate to the pharyngeal wall upon which he had operated. Discussion: Drs. Knight, Wright, Butts, Chappell, Simpson, Myles, and Mayer.

W. H. Park, M.D., spoke of a case of carcinoma of

the larynx. Discussion: Drs. Knight, Wright, and Simpson.

DECEMBER 27TH, 1893.—D. L. Hubbard, M.D., presented a larynx removed for intralaryngeal growth.

W. H. Park, M.D., showed a patient with tumor on the roof of the mouth. Case discussed by Dr. Knight.

W. H. Park, M.D., also showed a patient with laryngeal tumor and wearing a tracheotomy tube.

E. Mayer, M.D. Paper: "The treatment of follicular pharyngitis." Published, N. Y. Eye and Ear Infirm. Reports, vol. ii., part 1, 1894. Discussion: Drs. Lederman, Asch, Phillips, and Park.

W. H. Park, M.L. Paper: "The bacteria of the human mouth and their relation to throat inflammations." Discussion: Drs. Van der Poel, Chappell, Coffin, and Asch.

W. C. PHILLIPS, M.D.,
Secretary.

SECTION ON OBSTETRICS AND GYNÆCOLOGY.

JANUARY 26TH, 1893.—H. J. Garrigues, M.D., reported a case of symphysiotomy.

E. H. Grandin, M.D. Paper: "A clinical study of puerperal peritonitis." Published, Amer. Med.-Surg. Bull., 1893, vi., 184. Discussion: Drs. Von Ramdohr, Vineberg, Edebohls, and Collyer.

FEBRUARY 23D, 1893.—C. E. Nammack, M.D., reported a case of ruptured tubal pregnancy, and presented the specimen.

G. M. Edebohls, M.D., presented and reported a case of double uterus.

J. C. Edgar, M.D., presented a case of ventral fixation of the uterus following a puerperal peritonitis.

H. T. Hanks, M.D. Paper: "Pelvic inflammation following the puerperal state." Discussion: Drs. Murray, Jewett, Forest, Wylie, Grandin, and McLean.

MARCH 26TH, 1893.—J. C. Edgar, M.D., presented a fœtus with the cord coiled six times about the neck, and described the history of the case.

H. C. Coe, M.D., discussed the subject of symphysiotomy, and was followed by Drs. Jewett, A. P. Davis (Philadelphia), Grandin, Murray, and McLean.

APRIL 27TH, 1893.—J. E. Kelly, M.D. Paper: "The anatomy of the round ligaments." Not published.

Dr. Johnson (Boston) read a paper narrating one hundred and eighty-one cases of Alexander's operation, performed mostly in St. Elizabeth's Hospital, Boston. Discussion: Drs. Edebohls, Mundé, Buckmaster, Currier, and Vineberg.

MAY 28TH, 1893.—J. C. Edgar, M.D., reported and presented a case of symphysiotomy. Discussion: Drs. H. A. Kelly (Baltimore), Currier, and Hanks.

Dr. Peckham-Murray reported a case of hæmatoma of the vulva.

H. A. Kelly, M.D. (Baltimore). Paper: "Urinalysis in gynæcology." Published, Amer. Med.-Surg. Bull., 1893, vi., 934. Discussion: Drs. C. P. Noble (Philadelphia), W. H. Porter, and C. Heitzmann.

OCTOBER 26TH, 1893.—E. P. Davis, M.D. (Philadelphia). Paper: "Toxæmia of pregnancy: its diagnosis and treatment." Published, Amer. Jour. Med. Sci., 1894, cvii., 147. Discussion: Drs. Von Ramdohr, Grandin, Murray, Edgar, M. P. Jacobi, and Buckmaster.

NOVEMBER 26TH, 1893.—A. C. Stanard, M.D., presented specimens: 1. Fibromyomata of uterus with tubo-ovarian disease. 2. Primary abdominal peritoneal pregnancy. Discussion: Drs. Grandin and Currier.

A. F. Currier, M.D., presented specimens: 1. Small ovarian cyst with hydrosalpinx. 2. Dermoid cyst. Discussion: Drs. Waldo and Porter.

A. H. Goelet, M.D., presented specimens: 1. Small

uterine fibroid. 2. A method for checking hæmorrhage by electricity by means of specially constructed electrodes. Discussion: Drs. Porter and Currier.

DECEMBER 28TH, 1893.—S. Marx, M.D. Paper: "Accouchement forcé." Published, Med. Rec., 1894, xlv., 202. Discussion: Drs. Grandin, McLean, Edgar, and Murray.

G. M. Edebohls, M.D., demonstrated upon the blackboard methods for the radical cure of umbilical hernia.

H. C. Coe, M.D. Paper: "Hysterical manifestations due to alcohol." Published, Amer., Med.-Surg. Bull., 1894, xii., 135. Discussion: Dr. R. A. Murray.

J. C. EDGAR, M.D.,
Secretary.

SECTION ON PÆDIATRICS.

JANUARY 12TH, 1893.—

Discussion on Fractures in Infancy and Childhood.

R. H. Sayre, M.D. Paper: "Good results in fracture of the clavicle in 'run-about' children." Illustrated by patients. Published, Arch. Pædiat., 1893, x., 3.

B. F. Curtis, M.D. Paper: "Neglected fractures in children." Illustrated by a patient. Published, Med. Rec., 1893, xliii., 609.

R. H. M. Dawbarn, M.D. Paper: "Methods of treating fractures of the thigh in infants." Published, The Polyclinic, 1893, i., 93.

W. W. Van Arsdale, M.D. Demonstration: An apparatus for the treatment of fracture of the thigh in infants.

FEBRUARY 9TH, 1893.—J. L. Smith, M.D., presented a case of extreme deformity of the chest.

Mr. T. C. Janeway, of Yale University. Paper: "Pineapple juice as a solvent of diphtheritic membrane." Not published.

Report of cases by Drs. H. D. Chapin, J. E. Winters, J. L. Smith, and E. G. Janeway.

D. Brown, M.D. Paper: "Differential diagnosis of various forms of obstructed respiration." Not published.

T. H. Manley, M.D. Paper: "Surgical measures of relief in stenosis of the air passages." Published, Med. and Surg. Reporter, 1893, lxviii., 284.

MARCH 9TH, 1893.—W. L. Stowell, M.D., reported a case of pneumonia which had been followed by empyema, the diplococcus pneumoniæ having been found in the pus.

Discussion on Catarrhal Pneumonia.

H. Koplik, M.D. Paper: "Bacteriology." Not published.

F. Delafield, M.D., spoke on its "pathology."

J. E. Winters, M.D. Paper: "Symptomatology." Not published.

A. Jacobi, M.D. Paper: "Treatment."

S. Baruch, M.D., spoke on its "hydrotherapeutic treatment."

APRIL 13TH, 1893.—J. H. Fruitnight, M.D., presented a case of lateral curvature of the spine following empyema. Published, Archiv. Pædiat., 1893, x., 7.

Discussion on Convulsions in Young Children.

L. C. Gray, M.D. Paper: "Differential diagnosis of various forms of convulsions."

G. M. Hammond, M.D. Paper: "To what extent can epilepsy be prevented by early recognition and treatment?" Published, Med. Rec., 1893, xliii., 615.

H. W. Berg, M.D. Paper: "Pathology and treatment of simple eclampsia." Published, Amer. Med.-Surg. Bull., 1894, vii., 839.

MAY 11TH, 1893.—H. Koplik, M.D., presented a case of

facial paralysis with herpetic eruption; also a case of syphilis acquired from circumcision.

Discussion on Milk as an Infant Food.

J. E. Allen, M.D. Paper: "The milk supply of New York: its sources and adulterations." Not published.

R. G. Freeman, M.D. Paper: "The comparative merits of sterilization and Pasteurization in the preservation of cow's milk." Published, Med. Rec., 1893, xliii., 709.

Prof. R. H. Chittenden, of Yale University. Paper: "Papoid as a proteolytic agent in the digestion of milk." Not published.

OCTOBER 12TH, 1893.—

Discussion on Milk.

W. P. Northrup, M.D. Paper: "'Modified milk' by prescription laboratory." Published, Med. Rec., 1893, xliv., 552.

R. G. Freeman, M.D. Paper: "Contaminations of milk." Not published.

H. L. Coit, M.D., of Newark. Paper: "Clinical standards of cow's milk." Not published.

NOVEMBER 9TH, 1893.—H. Koplik, M.D., presented a new preparation of phosphorus.

Discussion on Intestinal Obstruction in Early Life.

W. L. Stowell, M.D. Presentation of specimen of congenital intestinal obstruction.

H. D. Chapin, M.D. Report of a case simulating intestinal obstruction.

J. Dorning, M.D. Paper: "Forms of intestinal obstruction in early life, with their diagnosis." Published, Med. Rec., 1894, xlv., 556.

J. L. Smith, M.D. Remarks on treatment.

B. F. Curtis, M.D. Paper: "Surgical measures of

relief." Published, Amer. Med.-Surg. Bull., 1894, vii., 5.

W. Meyer, M.D. Remarks on surgical treatment.

W. E. Forest, M.D. Remarks on treatment by graduated pressure.

DECEMBER 14TH, 1893.—H. Koplik, M.D., presented two cases representing a rare form of laryngeal spasm.

J. L. Smith, M.D., reported a case of septic infection following vaccination.

R. G. Freeman, M.D., reported the autopsy upon this case.

Discussion on Scarlet Fever.

W. H. Park, M.D. Paper: "Bacteriology."

J. W. Brannan, M.D. Paper: "State and municipal control of infectious and contagious diseases." Published, Med. Rec., 1893, xliv., 833.

W. H. Thomson, M.D. Remarks on infectious diseases.

A. Jacobi, M.D. Paper: "Treatment of scarlet fever." Not published.

F. M. CRANDALL, M.D.,
Secretary.

SECTION ON NEUROLOGY.

JANUARY 13TH, 1893.—J. Collins, M.D. Paper: "Hæmatomyelia and acute myelitis." Published, Med. Rec., 1893, xliii., 648. Discussion: Drs. Dana, Gray, Fisher, Manley, and M. P. Jacobi.

APRIL 14TH, 1893.—T. Manley, M.D. Paper: "Traumatic hæmorrhage into the medulla spinalis." Published, Jour. Nerv. and Mental Dis., 1893, xx., 484. Discussion: Drs. Dana, Collins, and Hammond.

A. M. Phelps, M.D. Paper: "Operative procedures on the spinal column for lesions involving the cord." Published, Jour. Nerv. and Mental Dis., 1893, xx., 467. Discussion: Drs. J. W. Wright,

Hammond, Fisher, Collins, M. P. Jacobi, Manley, and Sayre.

OCTOBER 13TH, 1893.—A. D. Rockwell, M.D. Paper: "Some causes and characteristics of neurasthenia." Published, N. Y. Med. Jour., 1893, lviii., 589. Discussion: Drs. Dana, Mason, Fisher, and M. P. Jacobi.

J. Collins, M.D. Paper: "Report of a case of progressive muscular atrophy associated with locomotor ataxia." Published, Jour. Nerv. and Mental Dis., 1894, xix., 92. Discussion: Drs. Dana and Fisher.

A. McL. Hamilton, M.D. Paper: "Insanities characterized by dominant concepts, with presentation of cases of an unusual form." Published, Med. News, 1894, lxiv., 57. Discussion: Drs. Mason, Fisher, Gray, and Dana.

Discussion on "Some recent experiences with hypnotism" was opened by Drs. Willy Meyer and Joseph Collins, and continued by Dr. M. P. Jacobi.

J. COLLINS, M.D.,
Secretary.

SECTION ON PUBLIC HEALTH, LEGAL MEDICINE, AND MEDICAL AND VITAL STATISTICS.

JANUARY 18TH, 1893.—G. M. Sternberg, M.D. Paper: "Disinfection at Quarantine, especially against cholera." Published, N. Y. Med. Jour., 1893, lvii., 57. Discussion: Drs. H. M. Biggs, E. O. Shakespeare, Gihons, and others.

FEBRUARY 1ST, 1893.—H. E. Crampton, M.D. Paper: "Municipal lodging houses in New York City." Discussion: Mr. J. A. Reis, Mr. Irwin, Drs. Dillingham and Derby.

MARCH 1ST, 1893.—Stephen Smith, M.D. Paper: "Proposed act to abolish the office of coroner in the City and County of New York." Discussion: Drs. A.

T. Weston, J. W. Brannan, S. T. Armstrong, E. G. Janeway, A. H. Smith, Hadden, and Vought.

Mr. E. W. Martin. Paper: "The present sanitary condition of the Croton water-shed." Discussion: Drs. H. D. Chapin, Derby, Cheesman, Hadden, A. H. Smith, E. G. Janeway, and Dillingham.

APRIL 19TH, 1893.—W. Vought, M.D. Paper: "Neglect of duty on the part of coroners under the present system." Not published.

A. Hadden, M.D. Paper: "Advocating that the powers of the coroners be enlarged." Discussion: Drs. Brannan, Kerchner, and others.

MAY 17TH, 1893.—C. G. Currier, M.D. Paper: "Patent medicines." Not published. Discussion: Drs. Eccles, W. H. Porter, Brannan, Ferguson, Bryan, and others.

OCTOBER 10TH, 1893.—E. Hart, M.D. Paper: "Quarantine: its merits, mischiefs, and mistakes." Not published.

NOVEMBER 30TH, 1893.—Mr. C. C. Savage. Paper: "Dispensaries, historically and locally considered." Published, Report of Demilt Dispensary, 1893; also in "Hospitals, Dispensaries, and Nursing," Chicago International Congress, 1893. Discussion: Drs. J. West Roosevelt, F. W. Jackson, Collins, H. D. Chapin, and S. T. Armstrong.

DECEMBER 20TH, 1893.—S. T. Armstrong, M.D. Paper: "The Academy of Medicine and Public Health." Published, N. Y. Med. Jour., 1894, lix., 353. Discussion: Dr. T. H. Manley.

F. H. DILLINGHAM, M.D.,
Secretary.

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